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THE ETHNOGRAPHY OF BRITISH NEW GUINEA.

I. THE ETHNOLOGICAL PROBLEMS.

IN the following pages I have endeavoured to put into a small compass the present state of our knowledge about the largest island in the world. The western half of New Guinea (long. 141°E.) belongs to the Netherlands, of the remainder the Germans have appropriated the northern half as far south as lat. 8°, while the English have annexed the southern half with all the neighbouring islands. The ethnography of this vast island is most interesting; but it is somewhat difficult to master, as there are so many books and papers bearing upon the subject, some of which are erroneous, others are worthless from this point of view, many contain a few interesting facts and much padding, while not a few are of great value.[1]

Many of those who have written on the natives of British New Guinea have not sufficiently distinguished between the numerous tribes in our possession, and they speak in vague terms of the Papuans as if they were all alike. Now this is by no means the case, and before we can gain an adequate comprehension of Papuan ethnography and ethnology we must clearly distinguish between the charac-

[1] The concluding section of this article, which will appear in an early number of "Science Progress," consists of a "Guide to the Literature on the Ethnography of British New Guinea". The references in the present section are to the bibliography in the following section.

teristics of the various tribes, their customs, languages and handicrafts.

There is still much discussion concerning the limitation of the term Papuan as applied to people, and even whether it should not be dropped altogether, as Prof. Sergi suggests. The Italian anthropologist extends the term Melanesian not only to comprise the natives of all the Western Oceanic islands including New Guinea and the adjacent islands but also Australia. At present I adhere to what Mr. Ray and myself have considered to be the most convenient course, and employ the term Papuan for what appear to be the autocthones of New Guinea. By Melanesians we understand the present inhabitants of the great chain of islands off the east of New Guinea and extending down to New Caledonia. These terms are used to designate peoples, not races; neither are pure races, and at present we are unable to gauge the amount of race mixture in either, or even to state precisely what are their components.

From the boundary of Netherlands New Guinea to Cape Possession on the eastern coast of the Papuan Gulf, and inland from these coasts, the natives are dark, frizzly-haired Papuans, typically they are a dolichocephalic people and rather short in stature.

The Papuans also occupy the greater part of the south-east peninsula of New Guinea; but along the southern coast line almost uninterruptedly from Cape Possession to the farthest island of the Louisiades is an immigrant Melanesian population, about whom I shall have more to say presently.

I will now enumerate a few facts which will clearly bring out the essential distinction between these two peoples.

We have not at present a sufficient amount of data on the physical characters of the two peoples by skilled observers to enable us to formulate what differences there may be between them. There is no doubt that the Papuans are more uniformly dark than are the Melanesians (I am now referring solely to the Melanesians in British New Guinea) and their hair is as constantly frizzly. Among the Melan-

esians light-coloured people, even as light as the true Polynesians, are constantly met with, as are also individuals with curly and occasionally straight hair. The features of the former are coarser and they are often much more prognathous; their skulls, too, are more generally dolichocephalic, whereas among the Melanesians a great variety is found in the cranial measurements, as Prof. Sergi has demonstrated. Judging from my experience of the Western Papuans and from numerous photographs I have seen, the Papuan men usually sit with their legs crossed under them like a tailor, whereas the Melanesians squat, like a Malay, usually with their haunches just off the ground. I do not know whether this rule holds good for the Papuans of the south-east peninsula.

The Western Papuans may or may not scarify their skin as in Torres Straits (Haddon, 1890, p. 366), Daudai (Beardmore, pp. 460, 468) and up the Fly River (Macgregor, *C. A.*, 105, 1890, pp. 43-51), but they do not tattoo; the Melanesians tattoo themselves, especially the women. Tattooing has, however, spread to a certain extent among the Papuan hill tribes of the peninsula, the Koitapu women appear to have thoroughly followed the fashion of their Motu neighbours; amongst the Koiari and other hill tribes it occurs only occasionally. The **V**-shaped chest mark, *gado*, occurs among the Motu and Loyalupu, but not east of Keppel Bay. Among the two former the tattooing lacks symmetry, but in Aroma curved lines become more frequent and asymmetrical figures have a bilateral symmetry with regard to the body. This subject was first dealt with by W. Y. Turner, Lawes followed, but the most exhaustive account is by Finsch (1885, *Samoafahrten*, and in Joest, *Die Tätowirung*).

The houses of the Gulf and Western Papuans are often of great size and contain numerous families, and there appears to be more club-life among the men. The houses of the Melanesians are smaller, each family possessing one. Very characteristic of the Papuans are the houses which are confined to the use of the men. The native name is *eramo*, *elamo* or *dubu*; by the white men they are variously termed

club-houses, sacred- or tabu-houses, temples, etc. These houses are the focus of the social life of the men, and as religion among savages is largely social usage, it is in connection also with these structures that most religious observances are held. Dubus apparently occur in the peninsula, but of these we have no precise information, and it is not recorded whether the carved platforms, which have been described at Tupuselei, belong to the social life of the Papuans of the district or their Melanesian conquerors.

The initiation of lads into manhood is accompanied with sacred ceremonies in some of the Papuan tribes, but so far as is known by none of the Melanesians in New Guinea. I have described the initiation ceremonies which took place in Torres Straits (1890, pp. 408, 432; 1893, p. 10), and in my forthcoming memoir (1894) I have quoted the information collected by Chalmers in the Papuan Gulf (1887, p. 85; 1890, p. 312). Masks are usually, perhaps invariably, worn at these ceremonies, and the bull-roarer is swung and shown to the lads; quite recently Chalmers has discovered the bull-roarer about the estuary of the Fly River as well as farther east (1887, p. 85; 1890, p. 313). The bull-roarer was also used in Torres Straits in connection with fishing and probably to raise a wind (Haddon, 1893, p. 20), now in some islands it is a children's toy (Haddon, *Jour. Roy. Inst. Gt. Britain*, 1890). There is no record of a bull-roarer among the Melanesian folk, but it occurs in the German territory (*Zeits. für Ethnol.*, xx., 1888 (Abhandl.), p. 267.

Kabadi and Nara girls about twelve or thirteen years of age are secluded for two or three years, they are sheltered from the sun and never allowed to descend from the house (Chalmers, 1885, p. 159; 1890, p. 319; *cf.* J. G. Frazer, *The Golden Bough*, ii., p. 228).

After initiation the lads rank as men and are allowed to marry if they can afford the bride price. Chalmers says the Gulf men have to pass several stages before the carved wooden belts can be worn; he also informs us that only old men have seen the sacred image of *Semese*, "and various are the initiatory steps before it can be seen" (1887, p. 86).

Masks are employed by many peoples during certain

ceremonies; their distribution in New Guinea is interesting, as it will be found that in the British Possession they characterise the Papuan as opposed to the Melanesian elements. They were common in Torres Straits, have been obtained in Daudai and are very abundant in the Papuan Gulf from Maclatchie Point to Cape Possession, they are probably in use along the remaining portion of the Gulf; the most southern limits at present known are the Maiva and Mekeo districts (St. Joseph River). Wilfred Powell, however, bought at Rogea (Heath Island) a turtle-shell dancing mask, which he was informed came from Mount Thompson on the mainland (long. 149° 50′E.); he describes and figures it on p. 16 of his *Wanderings in a Wild Country* (1883). This is the sole recorded exception to the statement just made, and must remain so pending further investigation. One or two specimens have been obtained from the neighbourhood of the Kaiserin Augusta River. Zimmermann says he saw them from Geelvink Bay to Humboldt Bay (*Internat. Arch. f. Ethnogr.*, ii., 1889, p. 53; *cf.* also Clercq and Schmeltz, p. 240). The Torres Straits and Daudai masks are made of wood or turtle-shell, those of the Papuan Gulf of a natural cloth which is painted and the designs are limned in cloissons; some of those of the German territory are somewhat similar to the latter, but the cloissons are, I believe, absent. What is known as to the occasions on which these masks are used will be found in Haddon, 1893 and 1894; in the latter Chalmers and others are quoted in full. In all cases masks are ceremonial and can only be worn during the ceremony. Photographic illustrations of eight Torres Straits masks, with careful descriptions, will be found in Meyer's *Masken*, etc. (1889), as well as of thirty-four small wooden face-masks from Taraway (Bertrand and Guilbert Islands), Kaiser Wilhelm's-Land (*cf.* also Partington's *Album;* Andree, R., "Die Masken in der Völkerkunde," *Arch. f. Anth.*, xvi., 1886, p. 477; *New Guinea*, p. 497). The Taraway masks are probably connected with ancestor-cult, like the *Karwars* of Netherlands New Guinea (*cf.* also *J. A. I.*, xix., pp. 319, 320).

Dancing may be a secular amusement or a ceremonial

exercise; in both aspects it is largely practised by the Papuans proper; the Melanesians do not appear to dance much, and then only for amusement. Religious dancing is not practised.

A classification and description of various kinds of dances in Torres Straits is given by Haddon (1893). Romilly (1889, p. 88) says: "In the south-eastern archipelago dances are very rare, nor have I ever seen one. Such dances as there are are only performed after a feast, no superstitious importance is attached to them; . . . but to the westward, in the Gulf of Papua, symbolic dances are constantly performed. . . . Each man is dressed to represent some bird or fish, and the dresses they appear in are marvels of ingenuity and construction. The shark is a very favourite symbol, as are also wild ducks and geese." A Motu dance is described on p. 54, and by Stone (p. 193), who also describes one at Hula (p. 195) and says: "Among the Motu the unmarried of both sexes join in the dance, but among the Kerapuno only the men"; but women dance close by at Kalo on some occasions (*Arch. Rev.*, 1890, p. 149), and Bevan (1890, p. 128) on an islet close by Dufaure Island saw men and women dancing surrounded by imitative children. Romilly notes (p. 60) a war-dance at Cloudy Bay. Chalmers (1887, p. 181) compares Motu and Motumotu dancing customs.

Social organisation is very simple in British New Guinea, there is no priestcraft or statecraft. In Torres Straits I discovered totemistic clans, which, however, appeared to be of diminishing importance, nothing of the sort has been described from elsewhere. The people are grouped into villages, and in some cases the inhabitants of various villages recognise one another as belonging to the same tribe. In the Port Moresby district members of the Koitapu and Motu tribes may live in apposition but not in fusion in the same village. Throughout the whole of the Protectorate chieftainship in the true sense of the term is absent; a man may acquire personal power through bravery, wealth or some other cause, but it is not hereditary, and even so these so-called "chiefs" have very little influence even

in their own village. The policy of the "Government" is to alter this in order to make some one more responsible for order in his district. The only exception to this state of affairs is in the Trobriand Group, where Macgregor found chiefs who exercise real authority.

Women take a good position in British New Guinea and exercise considerable influence over the men, less so, however, in the Trobriands.

In the foregoing social characteristics, in their lack of hospitality and in most of their mental traits the Papuans and Melanesians resemble one another, but the Papuans appear to be more noisy and excitable.

Of their weapons the stone-club is alone common to all the tribes. The use of the bow and arrow is confined to the Papuans and is universally employed to the west and in the Papuan Gulf, about Cape Blackwood the bamboo bow of the west is replaced by the palm-wood bow. Macgillivray (i., p. 29) alludes to the bow and arrow of Redscar Bay, Gill (1876, p. 249) says: "Their bows are made of wood, not of bamboo, as on the south-west coast, and are very inferior articles," and Moresby writes (p. 157): "As far as Redscar Bay (coming from the west) we saw toy bows amongst the children, but beyond that point the bow ceases altogether, till it reappears on the northern shores, west of Astrolabe Gulf". Turner (1878, p. 488) alludes to the bow and arrow of the Motu; but this is a mistake on his part.

Heavy, sword-like, wooden clubs are common among the Melanesians, and the sling is employed in the D'Entrecasteaux Islands. The throwing stick and javelin are used only by the western tribe of Torres Straits, who have adopted the practice from the Cape York natives.

Only the Melanesians make pottery, it is made at Yule Island (Navarre, p. 304) and at various places along the coast as far as Aroma (Gill, 1876, p. 249; Turner, 1878, p. 489; Chalmers, 1887, p. 23). In the islands off the south-east peninsula the pots are made in a different manner, the clay being laid down in a spiral and no stone and beater are

used.[1] Mr. Ray draws my attention to the fact that pots are called by the same name in Melanesia and British New Guinea (Fiji, *kuro;* Espiritu Santo, *kuro;* Motu and Maiva, *uro;* Kerepunu, *gulo*, etc.).

The Papuans earlier adopted tobacco and grew their own tobacco before the white man came, but they do not chew the betel to any great extent, quite the reverse is the case with the Melanesians. Strangely enough kava is only known along the Fly River and by the Masingara, a Daudai bush tribe.

In a memoir which is now in the press I discuss with considerable detail the decorative art of British New Guinea. I have been able to distinguish the following artistic provinces: i., Torres Straits and Daudai; ii., Fly River; iii., the Papuan Gulf; iv., Central District (*i.e.*, south-east peninsula from Cape Possession to Cloudy Bay); v., the Massims, including the district around Milne Bay and all the neighbouring island groups. The first three districts are purely Papuan, as is the bulk of the decorative art of the fourth, the Melanesians of this district appear to be an inartistic people; quite the reverse obtains for the island Melanesians, who are characterised by great skill in wood-carving and by the employment of varied and beautiful scroll designs. As this subject cannot be discussed in the absence of illustrations I must refer the reader to the original memoir.

I have now enumerated a sufficient body of evidence to demonstrate that two groups of people inhabit British New Guinea. We have now to see whether a further analysis is possible.

Our knowledge of the Western Papuans is too imperfect for any definite generalisations to be made at present, but I venture to present the following tentative suggestions:—

The most typical Papuans in the British Protectorate are probably the bush tribes from the Dutch boundary to the back of the Gulf of Papua. They are gradually being pushed

[1] *Cf.* Finsch, "Töpferei in Neu Guinea," *Zeits. f. Ethnol*, xiv., 1882, p. 574; *Samoafahrten*, p. 281; *Atlas*, pl. iv.. figs. 6-10; also H. O. Forbes in Haddon, 1894.

inwards by the coast people. Macfarlane contrasts the high and broad skull of the latter with the "long, narrow skull, with its low forehead and prominent zygomatic bones," of the former, whom he also states are "greatly inferior, both mentally and physically". The observations of D'Albertis[1] of a racial mixture in this region are supported by Quatrefages and Hamy (p. 256). The Torres Straits islanders are also a mixed people. I do not think we have sufficient evidence before us to decide what are the component races of these Western Papuans. I suspect that the Fly River is to a slight extent what may be termed a "culture route" and that the natives of the higher reaches have indirect communication with those of the north coast of New Guinea; for example, the rattan armour collected by D'Albertis high up the river is similar to that obtained by Finsch from Angriffs Havn, near Humboldt Bay, and recalls the coir armour of Micronesia; it is probable that this was the route by which tobacco found its way to Torres Straits and the Gulf district, and thence to the south-east.

The Papuans also extend down the south-east peninsula and into the adjacent island groups. On the mainland they have been conquered in certain places by Melanesian immigrants, and a mixture of these two peoples has taken place to a variable extent. In the islands the amalgamation has been more complete.

The immigrant people are by the great majority of writers spoken of as Polynesians; this identification is apparently based solely on the lighter colour of some of the former than that of the Papuans proper, and numerous words common to them and the Polynesians.

The light colour of the skin and the occasional presence of curly or even straight hair among some of the people of British New Guinea certainly proves a racial mixture, although Comrie and Finsch do not lay much stress on these points. The latter (1888, p. 234) writes: "The natives of Bentley Bay, as at East Cape, are of a tolerably

[1] ii., pp. 377, 381; *Jour. Anth. Inst.* vi., 1876, p. 216.

light skin colour and belong to what the ignorant would explain as a Malay mixture. But wrongly, for they are true Papuans, amongst whom the individual occurrence of curly, even of smooth hair is of no consequence." The craniology of the natives of the south-eastern peninsula and neighbouring islands has been studied by Comrie, Flower, Miklouchо, Maclay (*Proc. Linn. Soc., N.S.W.*, vi., 1882, p. 171), Quatrefages, Hamy and Sergi, most of whom admit with Flower "a considerable mixture of races among the inhabitants of this region of the world". As at present anthropography cannot speak with precision concerning the racial elements in this immigrant people we must turn to other branches of anthropology, and we will see what light ethnography and linguistics can throw on this ethnological problem.

A comparison of Papuan and Melanesian customs and handicrafts will prove that there is little of real importance in common, say, between the Motu or the South Cape natives and the Samoans. I need only allude to the almost total absence of a system of cosmogony or of a pantheon with a definite mythology; associated with this lack of a theology is the absence of an organised priestcraft. The democratic Papuans and Melanesians have no hereditary chieftainship, and the power of tabu is much more limited than in Polynesia. Strangely enough these so-called "Polynesians" in south-east New Guinea make pottery and do not drink kava.

For the linguistic evidence I have consulted my friend and colleague Mr. S. H. Ray, who is our great authority on the languages of Western Oceania, and the following account embodies certain conclusions, which he has kindly allowed me to quote from his unpublished MSS.: "It must be accepted as an axiom in philology and ethnology that the direction from which an observer approaches unknown languages or peoples will materially influence his description of them. The languages and customs of a newly discovered people will naturally be compared with one better known, and if there be any considerable amount of apparent agreement the conclusion will probably be hastily arrived

at that there is a connection between the newly found and the well known, and there will be a decided tendency to adopt the old as a standard and to refer the new to it. Those who first became acquainted with any New Guinea languages came from Samoa and Niue, where is found the fullest and most complete form of Polynesian speech." The numerous words which are common to the Motu (which is the only language of this district of New Guinea which has been thoroughly studied) and Polynesians "are also common in the Melanesian region. This leads to a further inquiry: Did the Motu and the Melanesians receive these words from the Eastern Polynesians? Before answering this it is necessary to observe that the Motu contains numerous words which are in the Melanesian of the Melanesian islands and do not appear in Samoan. A comparison of vocabularies shows that the Motu and the Melanesians stand in the same relation to the Samoan, whatever that relation may be." Mr. Ray shows that where Polynesian words diverge from Melanesian it is due to "worn down or abbreviated pronunciations of the fuller Melanesian, and the Motu has preserved a fuller form of certain words than the Samoan and hence presumably an older form. But there is a more important feature in the Motu language of New Guinea which distinguishes it from the Polynesian. Words are not used in the same way. The grammars of the languages are not essentially different, but in the Polynesian there is a tendency towards simplification which is not found in the Motu. It seems that we must regard the Samoan forms as in a later stage than that of the Melanesian. The Motu follows the Melanesian rule and is thus presumably in an older stage than the Samoan. Much more could be written to show that it is with the Melanesian tongues that the Motu of New Guinea should be included and not with the Polynesian. The same method applied to the Kerepunu, the Aroma, Suau, and other dialects akin to the Motu, points to the same relationship. The Motu grammar is entirely Melanesian and non-Polynesian. Such words as are common to it and the Eastern Poly-

nesian are equally common to the whole of Melanesia. Melanesian words which are non-Polynesian are also found in Motu and the allied languages of New Guinea."

I had long been puzzled by certain differences between the Motu and allied tribes on the coast of British New Guinea and the natives round Milne Bay and of the neighbouring groups of islands, all of whom I speak of collectively as the Massim.

There is a difference in their physiognomy. The Motu and allied tribes are remarkably destitute of a religion and are (or were) at the mercy of the sorcerers of the indigenous hill tribes, and, what is more remarkable, there is no trace of the cult of the sacred frigate bird or of that of any other animal. They make their pottery by beating a lump of clay into a pot, whereas, according to the only descriptions we have, the Massim women build up their pots with bands of clay laid on spirals. A study of my memoir on the decorative art of British New Guinea will clearly bring out the enormous difference between the Motu and the Massim in artistic feeling and execution.

My knowledge of Melanesia was too slight to enable me to proceed further with this problem, but in a recently published paper Mr. Ray says (1894, p. 32): "With regard to the place of origin of the Melanesian population of New Guinea it does not seem possible to ascertain the exact quarter from which it has come. There is at first sight much dissimilarity between the languages west and east, between the Motu and Kerepunu on the one side and the Suau of South Cape on the other. Though this dissimilarity disappears on closer examination, it may be stated that the language of Suau appears very similar to those of San Cristoval in the Solomon Islands, which lies almost due east of South Cape. The Motu and Kerepunu agree more with the languages of the Efate district in the Central New Hebrides."

Further evidence must be collected before Mr. Ray's suggestion can be definitely accepted. The decorative employment of the frigate bird in the Massims and Solomon Islands supports his first proposition; but, on the

other hand, inlaying with shell and nacre is very characteristic of the Solomon Islands and this is absent from the Massims, there are besides many other points of difference. So far as I am acquainted with photographs of natives from the New Hebrides I do not see any resemblance between them and the Motu, but it must be borne in mind that there can be culture-drift without appreciable actual mixture, though amongst savage peoples the latter must to a certain extent be concurrent.

When one thinks of the considerable amount of literature there already is on New Guinea one is surprised at the vagueness and imperfection of a good deal of our information. Now that the problems to be solved have been clearly stated it is to be hoped that further and more precise data will be forthcoming.

ALFRED C. HADDON.

WORK ON THE PALÆOZOIC ROCKS PUBLISHED IN 1894.

I. THE BRITISH ISLES.

IN reviewing the progress made in work amongst the palæozoic rocks, one is led to commence with a description of that which has been carried out amongst the rocks of our own country, though at first sight it might appear less necessary to allude to it, than to discuss the less accessible papers of foreign writers; still much of our own contribution lies partially buried in the transactions of local societies, and we have sufficient material to bring together in the form of a connected record, concerning one question of importance and interest to all, namely, the distribution of our coal deposits.

In this article it is proposed to notice such work as was published on the older rocks of our island during the first part of the current year, but the appearance of one paper on "Coal" in the August number of the *Journal of the Geological Society* requires notice out of its turn, as it will be convenient to consider it along with several other papers on this subject published during the earlier part of the year.

Before discussing the work of the first half of 1894, I feel that it is necessary to allude to my last article published in "SCIENCE PROGRESS," where I speak of the zonal method of working, as though it were a *new* method. Such is of course not the case. The subdivision of our strata into zones is merely a more detailed application of Smith's principle of strata identified by their included organisms, and no hard and fast line can be drawn between the early work of the pioneers of stratigraphical geology and the detailed work of the latest observers; indeed one can trace an insensible gradation from the one work to the other, and the later work could never have been done until the earlier was accomplished. Amongst the older rocks, the work of

Sedgwick and Murchison was followed by the more detailed work of Salter, Harkness and others, and especially of Hicks, and it was the fuller observations of these geologists which rendered possible the still more minute work of Lapworth and others, which brought about the results chronicled in my last article. The reference to the zonal method, as though it were a *method* instituted within the last fifteen years, was, therefore, hardly just to previous workers, and I gladly take this opportunity of correcting a statement which may have produced a false impression concerning the value of the researches of those workers, an impression which, I need hardly say, it was not my intention to produce.

Though much has been written upon the palæozoic rocks of Britain and their included fossils during the six months under notice, there is a relative dearth of papers which are strictly stratigraphical; but as many references are made to the characters of the rocks or fossils of the strata, which it is within my province to consider, brief allusion will be made to the more important of them, leaving their fuller consideration for the petrographer or the palæontologist. Papers which bear more or less directly on the nature of the Precambrian rocks are naturally numerous, and will be noticed, even when the Precambrian age of the rocks under consideration is a matter of uncertainty, for though the rocks may not be Precambrian, they may nevertheless throw some light upon the vexed questions connected with Precambrian rocks. Of the palæozoic rocks, we find that the lower division has not been the subject of many memoirs, whilst those treating of the upper division are, as might be expected, chiefly concerned with the carboniferous strata. It will be convenient to consider recent additions to our knowledge under three heads: to treat, in the first place, of those papers bearing upon the Precambrian rocks; next, of those which deal with the lower palæozoic rocks, and finally, of the more numerous ones which are concerned with the upper palæozoic strata.

1. *Precambrian Rocks.*

In dealing with these rocks, it is really the province of the stratigraphical geologist to treat of such as are known to be "stratified" and to leave the still mysterious "schists" alone, but in these days, when the latter rocks have so fascinated the minds of geologists, it is a hard task to pass them by. Perhaps, in the present state of our knowledge, it will be the best plan to point out what work has been done in each British area where rocks have been claimed as Precambrian, alluding only briefly to those regions where the age of the rocks has not been pretty clearly established. In doing so, we must pass along the length of the island from Cornwall to Ross, and may commence in the south, and travel northward, starting with the Lizard district. Awaiting with confidence for palæontological evidence concerning the age of these and other doubtful Cornish rocks, we pause for a moment only to refer to a paper which is rather petrographical than stratigraphical (1), in which the writer notices the existence of ultrabasic, basic, intermediate and acid intrusive igneous rocks, and argues in favour of their differentiation from an igneous magma. Travelling north from thence, we reach a spot where there is more satisfactory evidence of the Precambrian age of the rocks, for the Cambrian rocks are found in their immediate vicinity. This is the Malvern ridge. Here the publications of the six months under consideration deal with petrographical matter also, and a reference to the *Geological Magazine* (2) will enable the student of Archæan rocks to see two sides of a question as to the origin of certain biotites in the crystalline rocks of this ridge. Concerning the Scotch rocks Mr. Barrow has a paper (3), which to a large extent summarises work and views advanced in detail in a former paper which he read before the Geological Society, and a very acceptable contribution to our knowledge of the Precambrian rocks comes in the shape of the geological survey map of the district around Gairloch (4), so long celebrated on account of the relationship between the Torridon sandstone and the gneissose rocks in that region. Of the

older rocks, we find here foliated and massive hornblendic, pyroxenic and micaceous rocks, quartz rocks and quartz schists, limestones and graphite schists included in the legend under the name of "Lewisian Gneiss". These are interbanded with long strips of epidiorite running in a general north-west and south-east direction, and the whole succeeded unconformably by the "Torridonian" group, which the surveyors divide into a division of grey grits, dark shales, and breccias, and another of coarse red and chocolate grits. Lastly, Professor Bonney contributes a paper (5) on general questions concerning the formation of gneiss, in which are remarks bearing upon the formation of, or the occurrence of changes in, the rocks of Twthill (Caernarvon), Llanfaelog (Anglesea), the granite of St. Davids, the "Hebridean" rocks and Torridon sandstone of Scotland, and of rocks referred by some to the Precambrian, those of the Lizard district and of Guernsey.

2. *Lower Palæozoic Rocks.*

Contributions to the history of these rocks have been received from Cornwall and the northern part of England. In the former area Mr. F. Stephens maintains (6) that the rocks of the Marazion and Perranuthnoe districts, like those of St. Just, "must be considerably older than the strata of Camborne and Falmouth, possibly very early Cambrian," though no evidence appears in the paper in support of this view. A more important paper referring to the old rocks of this country is by Mr. Teall (7), who describes the occurrence of spheroidal greenstones with radiolarian cherts (these cherts occur both on Mullion Island, where they have been previously recorded in the publications of the Geological Society of London, and on the mainland in the Gorran and Veryan districts, where they have been subsequently detected by Mr. Howard Fox). On the mainland these cherts have been ascertained apparently to underlie the Caradoc quartzites. Mr. Teall mentions the record by Rothpletz of similar radiolarian cherts in Saxony associated with spheroidal diabases, and a similar association of ig-

neous rock and radiolarian chert has recently been detected in the Ordovician rocks of the southern uplands of Scotland. Jasper is associated with so-called spheroidal basalt in California, and Mr. Teall thinks that this jasper may possibly be a radiolarian deposit. He remarks that it seems "almost impossible to avoid the conclusion that the peculiar structures in question are in some way connected with the physical conditions under which the cherts accumulated," and asks if they may not be characteristic of submarine and possibly deep-sea lavas. Anything tending to throw light on the remarkably interesting palæozoic radiolarian deposits, which have been so largely detected of late years, is of interest, but it may be doubted whether it is not yet too early to generalise on the connection between radiolarian cherts and spheroidal basic rocks. The writer of this article finds in his note-books observations of spheroidal diabases in Bohemia in rocks of Precambrian, Ordovician, Silurian (Llandovery, Wenlock and Ludlow) and Devonian ages. In most cases they have broken along shales, and the association of radio larian cherts with soft graptolitic shales may account in some cases for the existence of the igneous rocks also in that position, for some at any rate of these Bohemian diabases are undoubtedly intrusive.

In a stratigraphical paper (8) on the Skiddaw slates of the North of England, the writer takes advantage of the observations of Harkness, Nicholson, Ward, Goodchild, Postlethwaite and others to summarise our present knowledge of these rocks; he considers that the Skiddaw slate group may consist of a thin series of Arenig rocks interfolded with older ones. He gives a list of graptolites compiled from previous lists with additions, and maintains that the presence of *Bryograptus* proves the existence of Tremadoc beds in the Skiddaw district. The paper also contains an attempt to subdivide the Arenig rocks of the Lake district and adjoining regions. A paper by Mr. Reed (9) contains a careful description of a new species of *Phacops* of the sub-genus *Chasmops* (*P. Marri*), from the Applethwaite division of the Coniston limestone of Applethwaite Common near Windermere. It is compared with other

forms, including the Russian species described by Fr. Schmidt, but the author gives reasons for separating it from these. Mr. Reynolds (10) adds to our knowledge of the fauna of the Lower Palæozoic rocks of Western Yorkshire, by describing a new species of trilobite rare in Britain under the name of *Dindymene Hughesiæ*. It is found in Bala rocks, which have also furnished a cystidean of the family *Anomalocystidæ*, on which the author has notes. He further records several trilobites from the Yorkshire Llandovery rocks which also occur in the Stockdale shales of the Lake district, and two species of *Cyphaspis* resembling continental forms and apparently new to Britain.

3. *Upper Palæozoic Rocks.*

We have few additions to our knowledge of the Devonian rocks, and these are hardly of such a nature as to require notice in an article written for stratigraphical geologists. Consequently in this portion of the article the carboniferous rocks alone will be dealt with, as the Permian rocks also have furnished no new material. Commencing with papers which are mainly concerned with lithological or palæontological details, it will be sufficient to allude, under the former head, to a paper which gives additional information as to the composition of fire clays (11), and under the latter, to a number of writings adding to our knowledge of the botany and zoology of the carboniferous period (12). The oil shales of Scotland receive notice in an American publication (13). They are noted as occurring in an area which is roughly twenty miles in diameter in the neighbourhood of Edinburgh, in the calciferous sandstone group, and have a thickness of 3000 feet. The shales are described, and their changes in the neighbourhood of intrusive igneous rocks, *e.g.*, the production of ozocerite in the Binney sandstone. It is stated that there is little doubt that this owes its origin to the distillation of bituminous matter by igneous intrusions in the vicinity of the overlying oil shale. The shale appears to have been formed in tranquil lagoons, into which vegetable matter was brought down in a very fine state of division, but that the hydrocarbon is

sometimes of animal origin is clear from the vast quantity of cyprids which make up some of the shales. Large plants are rare in these shales, but ferns are abundant.

In conclusion we have to deal with a very important group of papers treating of the distribution of coal under newer rocks, a subject that has received an unusual amount of attention of recent years. The first of these papers on the Forest of Wyre coal-field (14) is of somewhat local interest. It deals with the question of the distribution of the lower coal measures beneath the upper measures of that field. The author insists upon the value of the *Spirorbis* limestone, occurring in the upper coal measures, as a datum line. These upper coal measures, with "stinking" or "sulphur" coal, owing to the existence of the Symon Fault in this basin, rest unconformably upon the lower measures, with "sweet" coal resembling those of Coalbrookdale. He believes that the sweet coals only occur in the twelve thousand acres around Kinlet Knowle, in the Forest of Wyre coal-field, and are absent elsewhere, owing to the "Symon Fault". This "fault" he supposes to run in the direction of Shatterford, and maintains that on that account the "sweet" coals are absent there. The paper is illustrated by a map and sections. The next paper (15) treats of the important subject of the extension of the Yorkshire coal-field beneath the newer rocks to the east (15). It is illustrated with a map showing the outcrops of the Ganister, Silkstone and Barnsley coals in the Yorkshire, Derbyshire and Notts coal-field, and with sections. The author discusses the shape of the whole basin. "It would seem that the centre of the trough or coal-basin runs in a line from north-west to south-east, starting at the north-western outcrop of the coal measures, say at Denholme, about half-way between Halifax and Keighley, and proceeding south-west through Cleckheaton, Batley Carr, Sandal Castle (about two miles south of Wakefield), South Kirby and Doncaster. If this line were continued it would pass through Gainsborough to a point about seven miles north of Lincoln, and farther east to a point three or four miles south of Horncastle." He suggests the possibility of the easterly dip on the north-east

side of this axis being smaller than the western, in which case there would be a considerable extension of the eastern part of the basin below the newer rocks. It may be observed, however, that if the Pennine Chain, as is probable, is one of a series of monoclinal folds of which another is concealed to the east of this coal-field, the chances are that this concealed one has a steep westerly dip, which would considerably curtail the area of the basin in that direction. The author comments on the thickness of the newer rocks above the concealed coal-field, which would depend on the slope of the old post-carboniferous land surface, and the presence or absence of different members of the newer rocks, concerning which we require more information than the limited amount supplied by the Scarle boring. He refers to two other borings, details of which are not yet published: one at Carlton near Snaithe on the Aire, and the other between this and the Scarle boring on the Trent. In the discussion on this paper Mr. C. E. Rhodes, referring apparently to this Trent boring, says that a seam, assumed to be the Barnsley, was proved at a depth of 3300 feet, midway between Doncaster and Gainsborough.

The next group of papers refers to a question of still greater interest, namely, the possible existence of concealed coal-fields in the south-eastern counties of England. Mr. Harrison (16) has issued a pamphlet in three parts, referring to the possible existence of coal beneath the newer strata of Essex. The first part was written in 1887 and the second in 1891, whilst the third part is new. He suggests that the Ardennes-Mendip axis turns northward between Calais and Dover, and then in a westerly direction under London and the Thames Valley to the Mendip Hills, and that the Calais coal may lie to the north and the Dover coal to the south of this axis. Quoting the magnetic observations of Professors Thorpe and Rücker, he refers to the possibility of two ridges of older rock running northward from this axis, from near the town of Reading, the easterly one towards Cambridge, and advocates the desirability of searching for coal to the east of the latter ridge, in North-west Essex, in the neighbourhood of Quendon.

The Rev. A. Irving (17) doubts the existence of coal beneath that neighbourhood: he considers that the Ardennes-Mendip axis runs south of Dover, and that the old rocks beneath London form part of a ridge with a southerly slope, so that the centre of this ridge lay, not beneath London, but some distance to the northward, the older rocks of Ware, etc., forming part of it, and not of a north and south ridge running at right angles to it, and therefore coal is not likely to be found in Essex. He alludes to the carboniferous rocks occurring at Harwich, but considers that they may dip towards the south-east, so that the coal-basin may be under the sea, and not anywhere under the eastern counties north of London.

Messrs. Whitaker and Jukes-Browne (18) think there is nothing distinctive about the Palæozoic rocks reached in the Culford boring near Bury St. Edmunds, but remark that all are agreed that they are older than the coal measures. They believe that the Wenlock beds of the Ware boring are quite as likely to belong to Wenlock limestone as to the Wenlock shale series, and prefer to speak of them, therefore, merely as the Wenlock beds. In a section of the paper containing their general conclusions, they mention two points giving "slight support to the view that the old rock at Culford may be pre-carboniferous, and perhaps pre-Silurian in age. . . . On the other hand, however, the fact that at Harwich, which is nearer to Culford than any other of these deep borings, it is carboniferous slate that has been found, and that this is, in some respects, not unlike the harder parts of the Culford rock, naturally leads the sanguine to hope, if not to expect, that the latter too may be of carboniferous age. Should this view be right, the likelihood of still higher carboniferous and coal-bearing rocks occurring underground somewhere in the eastern counties is, of course, greatly increased." The authors also state that we can hardly expect that the northern rise of the older rocks continues underground beyond the neighbourhood of Culford. In the discussion upon this paper Prof. Boyd Dawkins called attention to the fact that the non-discovery of coal-bearing rocks in the area under consideration did not

prove the non-existence of coal-fields in the London basin, but merely of a pre-carboniferous region analogous to that separating the South Welsh and Forest of Dean coal-fields, or those of Gloucestershire and North Somerset. The Culford shale seemed to him probably of Silurian age. Professor Judd was not sure that the beds occurring at the bottom of the Richmond boring might not be abnormal carboniferous rocks, like those of the Northampton borings. The late president of the Geological Society, in the annual address, alluded to in my last article (19), gives a summary of our knowledge of the carboniferous rocks of the Dover boring, and remarks that "although the prospects of finding coal in the East Anglian Palæozoic area are not very bright, it is just possible that the adventurers may strike the coal measures in one or other of the narrow synclinal troughs running east and west in Essex, Suffolk and Norfolk". He himself is "disposed to agree with Mr. Brady that, in further explorations for coal beneath the secondary rocks, the southern alternative of Prestwich is the one which holds out the greatest hopes. It will be tolerably safe to assume that future operations should follow a nearly direct westerly course from Dover towards Bristol."

A few observations on the general question of the discovery of coal-fields in South-eastern England may not be out of place here.

The symmetry of the North of England coal-fields is very marked, though modified by a slight circumstance which produces a considerable deviation from true symmetry. The intersection of the Pennine anticline and Rossendale anticline nearly at right angles to one another produces a cruciform arrangement of coal-basins. On the east of the Pennines we have the Northumberland and Durham and the Yorks, Derby and Notts coal-fields, and on the west the Whitehaven and the South Lancashire and North Staffordshire. The dome of old rocks of the Lake district destroys the symmetry by an extensive modification of the Whitehaven coal-field, though the modification is perhaps not so marked as it appears, for these coal measures probably occupy a considerable area below the newer rocks of the Eden Valley.

Again the coal-fields of Southern England probably have a fairly symmetrical distribution on either side of the Mendip axis. But in Central England the case is different. It cannot be assumed that the Pennine Chain and the corresponding supposed anticline to the east of the Northumbrian and Yorkshire coal-fields run south to join the easterly continuation of the Mendip ridge, for their course would probably be profoundly modified by the existence of the old pre-carboniferous ridge running in a general east and west direction through Central England, which caused the absence of the lower carboniferous beds in that area. Such a barrier would certainly affect the subsequent folding, and as a consequence of it we actually see a marked want of symmetry in the arrangement of the exposed coal-fields of the midland counties. This want of symmetry might well, and probably would, extend to concealed coal-fields. Now the central ridge, according to Professor Green and others, should extend towards Bury St. Edmunds, and the old rocks of the Culford boring may be a portion of it. If this be the case, it would be very hard to predict the probable position of coal-fields in East Anglia, and indeed it seems to the writer that nothing short of an extensive series of borings can settle the question, and that advice as to the stations where borings should be tried is valueless, except in so far as it concerns the possible approximate thickness of rocks overlying the Palæozoic floor. The influence of this central ridge would not affect the old rocks south of the Ardennes-Mendip axis, and this seems an additional reason for advising trials to the south rather than to the north of that ridge. In the meantime the thanks of geologists are due to those who are patiently collecting the records of all borings and gradually aiding us in our restoration of the old buried palæozoic floor of South-eastern England.

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THE RESERVE MATERIALS OF PLANTS.

AT first sight the story of the metabolism of the vegetable organism seems to be that of a series of much simpler operations than those of the animal one. The food on which it subsists is presented to it in a very simple form, and the elaboration of the plant's substance from such compounds as are offered to it by the atmosphere and by the soil involves such a vast amount of constructive change that we may almost be pardoned for overlooking anything of a different nature. Yet it is equally true that vegetable protoplasm, like animal, is quite incapable of using these simple materials for its own nutrition and increase, in the forms in which we see the plant as a whole taking them in. The constructive changes, which we can trace, lead to the formation of vast numbers of bodies of various degrees of complexity and different organic constitution. But this construction must precede the actual nutrition of the living substance, and the food-stuffs on which the latter really depends are at least as complex as the food on which the animal lives. The structure of the vegetable organism favours the absorption of the simplest substances, and that chiefly because it is impossible for the elaborate food-stuffs of animal life to enter it. But these unelaborated materials do not nourish the plant without much expenditure of energy upon them. The essential similarity of the ultimate processes appears evident when we consider such organisms as the group of the Myxomycetes, where an approximation to the conditions of animal life is attended by a similar mode of absorption of food material.

The metabolism of the plant consists then of two essentially different processes. The simpler bodies originally absorbed are worked up at the expense of a great out-put of energy into bodies such as those on which an animal lives, and it is only after such formation that the actual nutrition of the organism can be said to commence.

The needs of the organism are again very similar. The

repair of the actual framework is not so great as that of the animal, but the continual formation of fresh material is comparatively greater. Growth, though slow in plants, taken as a whole, is nevertheless much more prolonged, and year by year sees also the formation of new substance to replace that cast off by reason of the changes of the seasons. The conditions of life are so different and the intermittence in the absorption of material so much more irregular that we must expect the whole story of metabolism to run upon different lines, though the ultimate nutritive processes may show a very close correspondence. The tendency of plants is to accumulate stores of material on which their protoplasm may subsist and from which they may construct their new substance. Long periods often occur in their lives during which their constructive powers are in abeyance, and as then no absorption of their food-stuffs can take place they must subsist on the stores they have been able to accumulate in more favourable times. The materials which thus are found deposited in their tissues have been called generally their *reserve materials*, and a great part of their metabolism is directed towards an accumulation of these.

We are apt, perhaps, in using this term *reserve materials* to think only, or chiefly at least, of those stores which we find laid up in seeds, tubers, or other parts to carry them over a period of absolute quiescence. This thought, however, falls short of the actual needs which they show. When we consider the processes of growth and repair we recognise that these are largely local and that the localities are many and widely distributed in the organism. The activity varies very much in these various regions from time to time, and with this variation arises the necessity for a continual circulation of the nutritive material about the plant. Besides the stores of food-stuffs deposited in the various reservoirs we can discover, we must recognise a circulating store, flowing sometimes in one direction and sometimes in another, consisting to a certain extent of the same materials as the quiescent reserves, but generally in a more plastic form, fitted, that is, for the immediate use of the living substance. Indeed the utilisation of the deeper reserves depends upon

their transformation into the forms found in the circulating fluid, under the influence of various ferments and other agents and under the control of the protoplasm found with the stores in the reservoirs.

In dealing with the materials which constitute the reserves of plant food we must therefore discriminate between these two descriptions of substances. Generally we find a distinction possible in the degree of solubility which attaches to them. The carbohydrates of the reservoirs are mainly starch or inulin; those of the circulating sap some form of sugar, or vegetable acids derived therefrom. The nitrogenous bodies of the seed are usually either definite grains or collections of amorphous proteids; those of the sap are mainly diffusible proteids or amide bodies largely derived from them.

In setting out to discuss the various forms of reserve materials, their modes of disposition and their fate, this must be borne in mind and the relations of the two classes recognised.

The kinds of material found in plants which belong to one or other of these categories are extremely numerous and varied. Their places of temporary deposit are very widespread, some charged with great abundance of nutritive substance and evidently set apart for a prolonged resting period; others containing a little only and this intended for only a short stay in its place of deposition. In seeds, tubers, bulbs, corms, fleshy roots and stems we recognise an almost permanent deposit; in medullary rays and cells of vascular tissue, in sieve tubes and laticiferous vessels, we find material laid down for only brief periods, ready for an almost immediate demand on the part of the living cells. In these cases we find each to have its most appropriate form, the temporary stores being as a rule of a more easily transformed character than the more permanent ones.

The nitrogenous reserve materials, in whichever of the two descriptions of reservoir we find them, are largely met with in the shape of some form or other of proteid matter. Till within recent years much obscurity existed as to vegetable proteids, many bodies with very curious reactions being

described by various writers, apparently showing no similarity to those of the animal organism. *Legumin*, *conglutin* and other names were applied to them, and their ultimate analyses showed but little agreement. The application of the various methods of examination which threw so much light upon the proteids of the animal body has, however, established the fact that they are of the same nature and of pretty nearly the same composition as the latter, the discrepancies at first so prominent being ascertained to be due to the modes of preparation or extraction.

Besides these proteid bodies, however, other nitrogenous compounds have been identified, chiefly, though not entirely, confined to the fluid sap which is in the living cells. These are of a much simpler composition; they include asparagin, glutamin, leucin, tyrosin, and other crystalline bodies which from their chemical constitution are grouped together under the name of *amides*. In their origin they are in many cases to be regarded as derived from the more stable proteids, and are indeed the chief form in which the latter are transported from place to place. They have, however, another possible source, being in many cases the expression of an incomplete constructive process and capable of conversion into the proteids themselves, from which later they are again reconstructed.

Some amount of nitrogenous reserve materials may also be found in some of the glucosides, such as amygdalin, present in the seeds and other parts of certain of the Rosaceæ, and myrosin, so prominent in many of the Cruciferæ.

Of non-nitrogenous reserve material we find the greatest amount to be carbohydrate. Here we find starch and its derivatives, cellulose, various kinds of sugar, inulin, and glycogen, giving a great variety of forms and great differences in amount in various plants. Next in importance come the glucosides, less uniformly distributed but still occurring very widely in particular groups of plants. Fats and oils are of common occurrence in many fruits and seeds, sometimes found alone, sometimes with carbohydrates in the same cells.

Besides these substances, of the meaning and import of

which there can be no doubt, we find many others the function of which is still a matter of controversy. Such bodies as tannin and phloroglucin are of fairly widespread occurrence and much is to be said both for and against their nutritive value. The vast number of alkaloidal matters offer a further opportunity for discussion, some facts in their history appearing to point to their possessing, at any rate, some importance in nutrition in particular cases. Lastly, we find in many plants small accumulations of mineral matter often associated with proteid deposits, though sometimes occurring separately, on which still much remains to be said.

In discussing the meaning of these various bodies, the peculiar relation of proteid substances to living protoplasm indicates their primary importance and they necessarily claim first attention. They are easily isolated, occurring as they do in large quantities in many seeds; and from their being found generally without a very large admixture of other bodies, they offer a fairly easy task to the analyst. In most works on vegetable physiology they are frequently spoken of in a general way under the name of *aleurone*, or aleurone grains. A section of a seed containing them shows them usually as rounded or oval bodies, sometimes presenting no particular structure, sometimes as containing crystalloid bodies and generally a small aggregation of mineral matter, which is usually a complex phosphate. Occasionally, as in the potato, they have the appearance of a cubical crystal, when the term aleurone grain is abandoned for *crystalloid*. Their original appearance in the cells in the seed has been investigated by several authors. Pfeffer (1) published in 1872 the first observations on record on this point. In his paper he attributes their first formation to the influence of the mineral constituent with which they are associated. The latter, usually either minute crystals of oxalate of calcium, or amorphous collections of the double phosphate of calcium and magnesium, can first be detected in the cell sap of the vacuole of the cell, and some peculiar attraction causes the proteid matter to accumulate round them and enclose them in its substance. As the seed

ripens the sap becomes less and less watery, and gradually the proteid is deposited by a process of precipitation till the complete aleurone grain is formed.

The mode of formation thus indicated seems to show that the process is altogether independent of the presence, or at least the activity, of the protoplasm of the cell. So important a matter as the storage of this form of reserve seems hardly likely to be altogether without the control of the living substance, particularly in the light of Schimper's observations on the mode of formation of starch grains in similar situations. Some careful researches carried out much later by Rendle (2) point to an altogether different mode of formation, at any rate in leguminous seeds, in which aleurone grains are extremely conspicuous. The material used by the latter observer was the seeds of species of *Lupinus*, his chief research being upon *L. polyphyllus*. The early stages of construction of the grains were only observable when the development of the cotyledons was sufficiently advanced to swell out the seed coat, by which time the endosperm had been absorbed. At this time the peripheral layer of protoplasm contained chlorophyll grains, in the substance of which starch was fast accumulating. While the store of carbohydrate food was thus being packed away the aleurone grains gradually became observable. Rendle lays stress on the fact that they first appeared as, or were preceded by, small bodies of spherical or ovoid shape which projected from the protoplasm. In some cells the latter only existed as a peripheral layer, in others it appeared also as bridles crossing the vacuole. Wherever the seat of formation could be noticed its first sign was this protoplasmic protrusion. Little by little these projections increased in size, growing inwards as well as outwards, till the grains could be seen as ovoid or spherical bodies embedded in the protoplasm, which, in consequence of their development, assumed the appearance of a coarse network. Gradually, as the process thus went on, the older grains increasing in size and newer ones being subsequently formed, the original vacuole became obliterated and the cell was swollen out by its own deposits. This mechanical process was accompanied

by a gradual change of a chemical nature. At first the deposit or secretion stained homogeneously much more deeply than the protoplasm; the grains dissolved in dilute caustic potash, but not completely, a membrane always being left. At this early stage they were not dissolved by sodium chloride either in ten per cent. or saturated solution, though they were soluble in these fluids when mature. As they advanced in development they became vacuolated, remaining always denser on their periphery. Finally they were again homogeneous and then showed the reactions of the grains of the ripe seed. Intermediate conditions could be observed as maturity was gradually approached.

Rendle thus argues that the process of aleurone grain formation is one of true secretion and not of a mere mechanical precipitation. He opposes Pfeffer's view of the part played by the mineral matter, observing that in *Lupinus* at any rate the cells contain no crystalline bodies at all at the stage when the grains begin to be formed.

Another view of the mode of their formation has been advanced. Wakker writing in 1888 (3) describes what he believes to take place in the endosperm of the castor oil plant (*Ricinus communis*). He notes that the protoplasm of the cells is greatly vacuolated, and suggests from his observations that each vacuole corresponds to an aleurone grain. This vacuole becomes filled little by little with the albuminoid matter as the seed ripens. Werminski (4), working on the same seed, in the main confirms his observations and supports his views. Van Tiegham has suggested from the firmness of the protoplasm round the vacuoles that the latter are not mere cavities in the living substance, but that they really correspond to plastids, and as they are filled generally with sap only they may be called *hydroplasts*. As the aleurone grains, according to Wakker's observations, are always formed in these vacuoles, we have a kind of plastid formation, something like the work of the amyloplasts.

This view has been criticised adversely by Ludtke (5), who in the main agrees with Rendle in regarding the process rather as one of intraprotoplasmic secretion. His

observations were made upon *Ricinus communis* and *Linum usitatissimum*. According to him both crystalloid and globoid originate in the protoplasm and not in the vacuole. He denies that the vacuole has any definite limiting membrane or that it has any particular value in the process. Sometimes he was able to observe a little space between the newly-formed grain and the neighbouring protoplasm, but nothing more definite than this. As the globoid, according to him, is very early to be observed, he goes back to a certain extent to the views of Pfeffer.

The secretion theory is opposed by Belzung (6), in a paper published in 1891, in which he supports the view of a mere mechanical precipitation much as Pfeffer did. The formation of the grains begins at a somewhat late period, when the cell sap is becoming more or less concentrated. The proteid or albuminoid principles, which will give rise to the grains, are, he says, partly dissolved in the sap by virtue of the alkaline phosphates which are present in solution and partly combined with soda or potash, in the form, therefore, of albuminates of these metals. Very weak solutions of the alkalis are well known to be appropriate solvents of these proteids. Weak acids precipitate the proteids from such combinations in the form of a whitish powder insoluble in water. At this point in the development of the seeds such weak acids, particularly citric acid, are present in the cells and their action causes a gradual precipitation, which proceeds until the aggregation of the precipitate can be seen to take the form of the completed grains. The progressive loss of water as the seed goes on to maturity causes the precipitation to be more abundant, so that the grains grow rapidly. Thus the formation is purely physical.

He considers his ideas confirmed by the ease with which artificial grains can be produced from solution of the proteids. Werminski obtained them thus from the material of *Ricinus*, and Bredow from that of *Lupinus luteus* by a process of dehydration.

Belzung's description of the way in which the aleurone grains first appear in the cells coincides fairly well with

the observations of Rendle, and seems rather opposed to his theory of their mechanical deposition. The formation begins late while the cells are a little short of their mature condition and the grains appear first at the periphery of the cells against the cell walls; that is, in the limiting layer of the protoplasm, and not, as a mechanical hypothesis would suggest, in the vacuole of the cell. After their first appearance they grow rapidly, and vacuoles form in their interior. The grains so appear to have a definite structure, consisting of a firm framework, enclosing within its meshes a peculiar sap, rich in various constituents of which proteid matter forms the chief part. The wall, or meshwork, is of different composition, being insoluble in water. The sap in its meshes is replaced as the seed ripens by proteid matter alone, except that organic acids, galactine, etc., may be detected in the latter in small quantity.

Belzung's work was carried out on leguminous seeds, chiefly the haricot bean, the common bean, the pea, the lupin and the broom.

In weighing the value of these several hypotheses as to aleurone grain formation and attempting to decide whether it is a vital process or no, it is clear that we have to take into account the fact that there is a copious accumulation of proteid matter in the cells as the seed is maturing. The appearance of the grains can be explained in two ways. Either they are formed in consequence of the transport of proteid in solution to the cells in such a condition as to be readily precipitated by the organic acids, etc., formed there, or they must be constructed from other plastic materials at the place where they are deposited. If they are transported in proteid form they must travel either as peptone or in the condition in which Belzung suggests they exist in the cell antecedently to their precipitation, that is, either as albuminates of an alkali metal, or in some state of solution with the phosphates in the sap. It is difficult to entertain either view; peptone is the only form of proteid which is at all easily diffusible through membranes, such as cell walls; but peptone has not been found in the cells during or before the deposition of the grains, nor would peptone be trans-

formed by weak organic acids into such proteids as are found in the aleurone. On the other hand, even if weak acids in the cell could cause a precipitation from the solution of the proteids in the second suggested condition, it is extremely unlikely that such solutions could pass readily from cell to cell, as must be the case for the accumulation of the aleurone grains. The hypothesis of protoplasmic secretion has much more to be said in its favour. It agrees with the way in which carbohydrate formation takes place, the latter being always the result of the work of a plastid or of protoplasm itself, as we shall see later. It explains also the large amount deposited, as the constructive activity of the living substance is at the time considerable.

Taking this view there are two modes again of regarding the process. The living substance may deposit it as a definite secretion from its own substance just as the protoplasm of other cells may deposit enzymes and other substances, or it may transform into proteid bodies other materials sent to the cells which, though not proteid, nevertheless are substances on the way to become so. That is, that the process of proteid construction, normally occurring in the leaves, may also take place in the reservoirs just as in the case of carbohydrates. This is a probable view, and is supported by the fact that when aleurone grain formation is beginning, amide bodies, and particularly asparagin and leucin and sometimes tyrosin, can be shown to be present in the cells. We know from other experiments that when the aleurone is being used in constructive processes during the germination of the seed such amides do appear as the result of the decompositions accompanying germination. Indeed, these bodies appear from many considerations to be the form in which the transport of nitrogenous nutritive material takes place in the plant. Their occurrence in the cells, therefore, while deposition of aleurone is taking place, indicates them as a probable immediate antecedent of the grains themselves. If this is so, then we must take the view of protoplasmic construction and not mechanical deposition, as we are not able, apart from

the influence of living substance, to convert amides into proteids.

We may now examine the aleurone grains with a view to ascertaining what proteids they are composed of. These are found to vary in different seeds, but the variety is not very great. The classification of them turns upon their solubility in various solutions or extracting fluids. On this point most of our information is derived from the investigations of Weyl (7) and of Vines (8). The former observer experimented on the seeds of oats, maize, peas, almonds and mustard, and on Brazil nuts. He extracted his seeds first with water and subsequently with solutions of neutral salts of various strengths. His watery extracts contained certain bodies which have since been shown to belong to the group of the albumoses; his salt solutions extracted two globulin bodies showing different properties. The strength of solution of most value was 10 per cent. and the salt was sodium chloride. The extract of the seeds prepared with this solvent was found to give a copious precipitate or coagulum on boiling. Further experiments showed that it contained two globulins, one of which was thrown down by saturating the solution at the ordinary temperature with crystals of common salt while the other remained dissolved in the concentrated solution. On heating solutions of these two proteids separately they were found to coagulate at different temperatures, the first at 55° to 60°C., the other at 73°C. Weyl gave the name of vegetable myosin to the former and that of vegetable vitellin to the latter.

The work of Vines carried the matter further. Besides working on quantities of the seeds and extracting the proteids on a large scale he examined the action of the solvent fluids upon sections of the seeds under the microscope. His most interesting results were obtained with the grains of the castor oil seed and those of the Brazil nut. In these cases the grains are of complex composition, showing a well-defined crystalloid and globoid in the interior of each. When a section of the endosperm of the castor oil bean is examined, after removal by alcohol of the oil it contains the

aleurone grains are seen as opaque oval bodies lying in the dense protoplasm. Water passed through the preparation dissolves a portion of the exterior, making them transparent and bringing to view the crystalloid and globoid in their interior. Lavage of the section with 10 per cent. solution of NaCl dissolves the rest of the grain, leaving the crystalloid and globoid free. Saturated salt solution then dissolves the crystalloid. The globoid, which is mineral in character, being composed of the double phosphate of calcium and magnesium, remains alone in the cavity where the original aleurone grain had been.

Vines examined a very large number of seeds by this method and in the main confirmed Weyl as to the composition of the aleurone grains. In some seeds, particularly in those of *Sparganium racemosum*, he discovered another proteid belonging to the class of albuminates or derived albumins, soluble only in dilute acids or alkalis. This proteid he found to occupy the external portion of the aleurone grain.

We have thus in these seeds two members of the globulin family differing in their degree of solubility in neutral salt solutions and in the temperature at which they are changed by heating. The proteid soluble in water was called vegetable *hemialbumose;* it has since been shown to be a mixture consisting chiefly of protalbumose and heteroalbumose. The characteristic reaction which is given by both these bodies and hence by Vines' watery extract is that the proteid is precipitated by nitric acid and that the precipitate is dissolved by warming the liquid, and thrown down again as the solution cools.

Following on the investigations of Vines, but some few years later, we have the results of Martin's examination of the seeds of *Abrus precatorius* (9), the jequirity or Indian liquorice, which are toxic in their properties. The proteids were extracted from the ground seeds by soaking the powder for twenty-four hours with 15 per cent. solution of sodium chloride, which was then filtered off. The extract was saturated with ammonium sulphate, which precipitated them. They were found to be two in number, a globulin

and an albumose. The former is readily soluble in 10 to 15 per cent. sodium chloride solutions and coagulates on heating to about 70°C. The albumose is nearly identical with Kuhne's deutero-albumose and appears to be the same body as the same observer discovered to exist in the latex of the Papau (*Carica papaya*). It will be described later.

As already mentioned, besides aleurone grains, proteid has been found in definite bodies in the cells of the tuber of the potato, where it exists in the shape of crystals of cubical form. These are insoluble in 10 per cent. solution of sodium chloride, but dissolve at once in saturated solutions. They belong, therefore, to Weyl's vegetable vitellin. Zöller has found (10) in the potato also some vegetable myosin, which is soluble in 10 per cent. sodic chloride solution. This is probably in the form of amorphous proteid in the protoplasm of the cells, as the microscope does not reveal its presence. Other observers have pursued the same line of investigation, and with the better means of examination that have been devised in recent years have made still further determinations of the nature of the proteids of the seed. Chittenden and Osborne (11) in 1891 discovered that the maize contains more than one globulin and a mixture of albumins, the latter being soluble in distilled water, and the solution coagulating on boiling, therein showing a difference from the albumoses, which are not so changed. Besides these they describe a peculiar proteid which differs from all others so far found by being insoluble in water, but soluble on warming with dilute alcohol. To this they have given the name of *zein*. The action of alcohol on the grains was shown by Vines to be somewhat peculiar, many of the crystalloids being insoluble in saturated solution of sodium chloride until after treatment with alcohol, when they dissolved readily.

The occurrence of proteid in some such forms as some of these was indicated by Zacharias (12) in sieve tubes, where it became coagulated on heating. Fischer (13) made a similar discovery in Cucurbita. Zacharias describes his proteid as insoluble in water or neutral salts,

but soluble in dilute acids and alkalis. It is coagulated by contact with alcohol and gives the usual chemical reactions.

Besides the stores of proteids thus evidently reserve materials deposited for use after a considerable resting period, we find other stores more immediately available for nutritive purposes. In many plants definite channels in the tissue are stored with a fluid of considerable complexity, known generally as latex. Probably this fluid contains many bodies which are not to be regarded as constructive materials, but rather as in some sense waste products, or at any rate as bye-products of metabolism. Besides these, however, are many plastic substances, such as starch, sugar, etc., and most latex contains considerable quantities of proteids.

Martin (14) has investigated with great care the latex of the Papau (*Carica papaya*), working, however, on the dried juice. He has proved that this fluid contains two well-defined albumoses, a globulin and an albumin. To them he attributes the following reactions :—

Globulin.—Soluble only in solutions of neutral salts, and precipitated from such solutions by saturation with either sodium chloride or magnesium sulphate.

Precipitated from its solutions by large dilution followed by the passing of a stream of CO_2 through the dilute extract for some time.

Precipitated by dialysing till the neutral salts are removed.

Coagulated on heating the solution in 10 per cent. sodium chloride to 70 to 74°C.

This globulin resembles Weyl's vegetable myosin in most respects, but its coagulation temperature is considerably higher.

Albumin.—Soluble in water and not precipitated by saturation with neutral salts. Coagulated in heating and on addition of nitric acid.

αPhytalbumose. — Soluble in cold or boiling water, not precipitated from neutral solution by NaCl or $MgSO_4$ but thrown down after long shaking in slightly acid solution. Martin considers this body to be much like Kuhne's protalbumose (15). It is probably a constituent of Vines' hemialbumose.

βPhytalbumose.—Soluble in cold water; precipitated by heat in two stages, the first precipitate falling at 78° to 82°C. and the second at 83° to 95°C.

Not precipitated by dialysis.

Both these albumoses give the characteristic behaviour of their nitric acid precipitate.

Martin points out the resemblance between the latter body and Kuhne's hetero-albumose.

In 1885 the writer (16) had the opportunity of examining the latex of several trees, particularly *Mimusops globosa*, *Parameria glandulifera* and *Brosimum galactodendron*. *Mimusops globosa* is the source of the gum balata of British Guiana. Its latex contains an albumose which is soluble in distilled water, and is not coagulated by boiling in neutral solution. At a temperature approaching 70°C. it is slowly thrown down by nitric acid. It is not precipitated by acetic acid and potassic ferrocyanide.

Parameria is a member of the Sapotaceæ. Its latex contains a body more like a peptone than any of the foregoing, being capable of dialysis. It does not, however, give easily the biuret reaction so characteristic of the latter group. It is convertible into peptone by the action of pepsin.

Brosimum galactodendron is the so-called "cow-tree" of South America and is a member of the Artocarpeæ. It has been known to yield a rich milky latex since 1823, when it was the subject of a paper by Boussingault and Mariano de Rivero, in which attention was called to its containing what the authors regarded as vegetable fibrin. The chief proteid present in the latex is an albumin, which is soluble in water and coagulates on heating to 68°C. or by treatment with alcohol. After removal of this body the latex

is found to yield the same peptone-like body found in Parameria.

Besides the laticiferous tissue we find other parts of plants, especially the succulent cortex of stems, the parenchyma of fleshy roots, etc., the seat of proteid stores. These are not necessarily reserve materials, being possibly plastic material on its way to be used up in constructive processes. In the large fleshy roots of biennial plants and possibly also in succulent perennial roots the contents of the cells must however be regarded as true reserves.

In the cortex of the stem of the Manihot there is to be found a globulin a good deal like those already described. It is insoluble in water but soluble in solutions of neutral salts. From the latter it can be precipitated by dialysis, by large dilution or by saturation with solid $MgSO_4$. Its coagulation temperature in neutral solutions is 74° to 76°C.

In the succulent tissue of the lettuce (*Lactuca sativa*) there is an albumose which is soluble in distilled water and is not coagulated on boiling, but is precipitated by nitric acid or by acetic acid in the presence of potassic ferrocyanide.

But little work has been done upon fleshy roots, but an investigation made recently on the asparagus (17) shows in the underground parts of this plant an albumin which gives the characteristic reactions of that group.

The proteids found in latex and in the various succulent tissues described are not in the forms of the aleurone grains of seeds. They are either in solution in the complex sap of the cells of the tissue, or are packed away in amorphous form in the meshes of the protoplasm. If cells of these parts are treated with alcohol under the microscope the clear, transparent vacuoles become turbid and opaque, indicating a precipitation of the proteid by the spirit.

The list of proteids so far discovered may be supplemented by true peptone, which can be determined in germinating seeds both of leguminous plants (18) and of cereals.

This peptone is probably, however, but a stage in the transformation of the resting reserve material to amide bodies which are transported from the food reservoirs to the seats of active growth.

So far, then, the reserve proteids proper are non-diffusible, granular or amorphous bodies, with difficulty soluble and almost without power of dialysis. They include several forms which show differences in these respects. There are several albumoses, soluble much more readily than the other kinds and apparently nearest in constitution to the diffusible peptones; globulins, comprising members of two groups which show great differences in their behaviour to heat, the one coagulating between 50° and 60°C., the other between 70° and 80°; and albumins. In regions where storage is not so evident we have in addition members of the peptone group, giving characteristic reactions and capable of dialysis.

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(To be continued.)

INDIAN GEOLOGY.[1]

IT is the misfortune of the stratigraphical geologist that the interest which is taken in his work varies inversely as the distance of the scene of his labours. Indian geology therefore stands but a poor chance of recognition in England; and the Indian geologist has only the melancholy satisfaction of knowing that he writes for posterity.

This ought not to be. There is much in the geology of India which can never be of interest outside of India itself; but there is much also which bears on the widest and greatest questions of the science. It is in India and in the southern hemisphere that we find the clearest evidence of a period of severe cold towards the close of the palæozoic era—evidence which only those who have not seen it have dared to doubt. It is in India and on the frontiers of India that we have the grandest mountain-chains of the globe, where the problems of mountain-building may frequently be studied without the vexatious hindrance of vegetative growth. In India, too, there are the great lava-flows of the Deccan Trap, and the magnificent mass of ancient crystalline rocks in Southern India;—but the geologist does not boast of these, for they have never been adequately examined.

The only general account of the geology of India which we had until lately was the *Manual* by Medlicott and Blanford, published in 1879. But Indian geology has not stood still since then, and the new edition of this *Manual* (edited and largely rewritten by R. D. Oldham) which has recently appeared shows how great is the progress that has been made. It is no bad record for the labours of a dozen men who are scattered over the whole of India and the adjoining countries, and who are now required by a short-sighted Government to devote the greater part of

[1] *A Manual of the Geology of India.* Chiefly compiled from the observations of the Geological Survey. *Stratigraphical and Structural Geology.* Second edition, revised and largely rewritten by R. D. Oldham. Calcutta, 1893.

their energies to inquiries of a purely economic nature. But it is deeply to be regretted that palæontological research has not kept pace with stratigraphical, for the work of the survey would be of infinitely more value to other geologists if the fossils collected were always properly compared and described.

When the first edition of the *Manual* was published, our knowledge was so fragmentary that it was impossible to give a connected account of the geology of India as a whole; and it was found necessary to break up the book into a series of descriptions of different districts. But so great has been the progress made since that time, that Oldham has been able to adopt a chronological arrangement; and although in many cases the chronology is still very doubtful, this plan greatly facilitates the comprehension of the work.

In this article it is my object to draw attention to some of the more important of the recent advances in Indian geology. Fuller information on all the points raised will be found in Oldham's *Manual*, except in the case of one or two observations which have appeared since the publication of that work; and references to the more important of the papers dealing with the questions discussed are given in the footnotes.

Few parts of the world can boast a finer development of "gneissic rocks" than Southern India; and no more promising field can be found for their investigation. But unfortunately it was in the early days of the Geological Survey, when petrology was still in its infancy, that this part of India was chiefly studied; and at that time it was almost universally held that gneiss had been formed by the metamorphism of ordinary sedimentary strata. Little attention was paid to these rocks, and on the map accompanying the first edition of the *Manual* they were all grouped under one colour.

Foote was the first to show that it was possible to distinguish a more schistose series, consisting of conglomerates, schists, hæmatite-beds and lava-flows, from the more truly

gneissic rocks; and to this series he gave the name of "Dharwar".[1] It forms long bands, or rows of elliptical patches, which run from N.N.W. to S.S.E.; and economically it is of great importance, for all the quartz reefs which bear gold in paying quantities occur within it. Since quartz reefs are equally abundant in the gneiss, it is most natural to suppose that the gold lay originally in the Dharwar rocks and has since collected by some process of "lateral secretion" in the reefs which now traverse them. And since the Dharwars consist largely of lava-flows, we may presume that the gold was brought up from below in those flows.

The Dharwar rocks invariably overlie the gneiss and are evidently newer than the greater part of it; but the relations of the two are not quite simple. Often near their borders the Dharwar bands are penetrated by masses and veins of gneissic rock; and this alone is sufficient to suggest that some parts of the gneissic system are eruptive and newer than the rest. Subsequent researches have tended to confirm this view, and the matter awaits further investigation.[2] It is unfortunate that there are no detailed petrological observations on the gneiss of India except those of Lacroix,[3] which were based on collections made many years ago in the Salem district. One of the most remarkable peculiarities, which has been noticed by several observers in various parts of India, is the abundance of hypersthene in these rocks.

In the Peninsula the gneisses and Dharwars are succeeded by a great thickness of unfossiliferous rocks, and no remains of life appear in any beds older than the Gondwanas (Carboniferous-Jurassic), but in the Punjab an un-

[1] See especially R. B. Foote. "The Dharwar System, the chief auriferous rock series in South India". *Rec. Geol. Surv. India*, vol. xxi. (1888), p. 40; vol. xxii. (1889), p. 17.

[2] Referred to in *Rec. Geol. Surv. India*, xxvi. (1893), p. 172; xxvii. (1894), p. 7.

[3] "Contributions à l'étude des gneiss à pyroxène et des roches à wernérite." *Bull. Soc. Franc. Min.*, 1889. Translated, so far as it relates to India, in *Rec. Geol. Surv. India*, xxiv. (1891), p. 155.

doubted Cambrian fauna has recently been discovered in the rocks of the Salt Range.[1] The most interesting forms are trilobites, of which two have been described under the names *Conocephalites Warthi* and *Olenus indicus;* but the reference of the latter to the genus *Olenus* is very doubtful. According to most observers the "Salt Marl" is older than the beds in which these fossils occur, but Middlemiss has attempted to show that it is not a sedimentary deposit but is of hypogene origin and has been intruded into its present position.[2] The proof of this, however, is not complete.

Beds which are referred to the Cambrian and Silurian are also found in the Himalayas;[3] but the fossils which occur in them have not been determined. Near Mandalay also, Silurian beds with *Echinosphærites* are stated to be present;[4] but this requires further confirmation.

It is not till we reach the Carboniferous that we find beds in which fossils are at all widely spread. In the Peninsula the fossiliferous part of the geological sequence commences with the great Gondwana system, the deposition of which appears to have begun towards the close of the Carboniferous period. It is divided into two groups,—the lower characterised by Equisetaceæ and ferns of the *Glossopteris* type; and the upper by Cycadaceæ.

At the time of the publication of the first edition of the *Manual* the age of these beds was one of the burning questions of Indian geology.[5] The difficulty of correlation

[1] King. "Note on the discovery of Trilobites by Dr. H. Warth in the Neobolus beds of the Salt Range." *Rec. Geol. Surv. India,* xxii. (1889), p. 153. Waagen. "Salt Range Fossils." *Pal. Ind.*, ser. B, vol. iv., pp. 92, 104.

[2] Middlemiss. "Notes on the Geology of the Salt Range of the Punjab, with a re-considered theory of the origin and age of the Salt Marl." *Rec. Geol. Surv. India*, xxiv. (1891), p. 19.

[3] Griesbach. "Geology of the Central Himalayas." *Mem. Geol. Surv. India*, xxiii. (1891).

[4] Noetling. "Field notes from the Shan Hills (Upper Burma)." *Rec. Geol. Surv. India,* xxiii. (1890), p. 78. See also Noetling. *Ibid.*, xxiv (1891), p. 104.

[5] The literature of the subject is extensive but has lost its former interest, and no reference is needed but to Oldham's admirable summary in the *Manual*, second edition, chap. viii.

arises from the fact that the Gondwana flora and fauna consist almost entirely of land and freshwater forms, very few of which have ever lived in Europe; and it is only indirectly that it has been possible to compare the Indian with the European sequence. Fortunately, in Australia, beds with plant-remains closely resembling those of India are associated with others which contain a marine fauna; and these marine beds can be correlated with the Carboniferous of Europe.

But this is not all. The Talchir group at the base of the Gondwana system in India, consists largely of a fine silt or clay; and scattered irregularly through the clay lie numbers of boulders of all sizes. In many cases the boulders must have been brought from a distance; for the rock of which they are made, is unknown in the neighbourhood. Occasionally too they are scratched and striated;[1] and the deposit was evidently ice-borne.

Similarly in New South Wales the marine Carboniferous beds consist of fine sand or shale in which are embedded subangular blocks of all sizes, some of which are striated. Intercalated among these beds are the Lower Coal Measures of Stony Creek, which contain *Glossopteris* and other Lower Gondwana forms; and above them come the Newcastle Coal Measures, the plants of which are for the most part identical with those of the Damudas, which in India succeed the boulder-bearing beds.

In the face of such evidence as this, it is impossible to doubt that the Indian and Australian beds were contemporaneous. The Australian beds can be correlated by their marine fauna with the Carboniferous of Europe; and the conclusion is irresistible that the deposition of the Gondwana beds of India began at about that period, and this conclusion is in harmony, as will appear presently, with observations which have been made in the Salt Range.

With regard to the upper limit of the Gondwana period, the evidence is not so clear. In the Umia group of Cutch,

[1] Fedden. "On the evidences of 'ground-ice' in tropical India, during the Talchir period." *Rec. Geol. Surv. India*, viii. (1875), p. 16.

plant-bearing beds are associated with the normal marine deposits of the group, and among the plants are several which are found elsewhere in the highest beds of the Gondwana system. Hence we may conclude that the Umia beds are approximately on the same horizon as the uppermost Gondwanas. Now the Umia plant-beds overlie beds which contain *Ammonites tomephorus*, *A. eudichotomus* (Tithonian forms), and *A. suprajurensis* and *A. bleideri* (Portlandian forms). Evidently therefore they belong to the very end of the Jurassic period; and the Gondwana system came to a close at about the same time. This being so, it is remarkable that many of the plants of the highest Gondwanas are said to be identical with, or closely to resemble, forms from the Lower Oolite of England. It is greatly to be regretted that the marine fossils associated with the Gondwanas of the east coast have not been more closely examined.

Turning now to the north, the Silurian beds of the Salt Range are overlaid unconformably by a group of sandstones known as the "Speckled Sandstone," at the base of which there is almost always a boulder-bed with striated boulders of rock which have come from a distance. The analogy with the Talchir deposit naturally suggests itself at once. No plants have been found; but in the beds immediately overlying the boulder-bed, *Conularia* and other marine fossils occur.[1] Thirteen out of the twenty-two species are identical with forms from the marine Carboniferous of New South Wales, which is also associated with a boulder-bed (already correlated with the Talchir boulder-bed). Only the most confirmed sceptic—with views of his own—can doubt that the boulder-beds of the Salt Range, of the Talchirs, and of New South Wales are on the same horizon; and since a similar bed, also associated with a *Glossopteris*

[1] Waagen. "Note on some Palæozoic Fossils recently collected by Dr. H. Warth in the Olive Group of the Salt Range." *Rec. Geol. Surv. India*, xix. (1886), p. 22. It was at one time doubted whether these fossils were contemporaneous with the rocks in which they occur; but this doubt has vanished in the face of further evidence.

flora, is found in South Africa, it is difficult to resist the belief that around the Indian Ocean at least, there was a period of severe cold towards the close of the Palæozoic era.[1]

The speckled sandstone of the Salt Range is succeeded by a great thickness of deposit, mostly calcareous, which contains one of the most interesting faunas of India. According to its fossils the upper part of the speckled sandstone itself must be included in this series. It is the Productus Limestone group [2] of Waagen, and, with his limitations, not a single species passes from it either into the beds below or into the beds above.

As already remarked, the lower part of the speckled sandstone contains fossils which are for the most part identical with those of the marine Carboniferous of New South Wales. But not one of these passes up into the beds above; and the fauna of the Productus group shows European rather than Australian affinities. It would lead us too far to attempt the correlation of the various subdivisions of the group, and it must suffice to say that the lower beds appear to be on the horizon of the upper Carboniferous or of the Permo-Carboniferous of Russian geologists (Artinsk stage of the Urals, etc.); while the upper beds are probably a little newer than the Permian. The most interesting feature of the whole group is the presence of true ammonites along with Palæozoic brachiopods.

The Productus Limestone group is succeeded by beds with Ceratites, and lithologically these might well belong to the same group. Indeed, originally the two were not distinguished, and the collections which have been made from the Ceratite beds since they were separated, have not yet been examined. A preliminary note upon them, however, has recently been published by Waagen,[3] from which

[1] Waagen. "Die Carbone Eiszeit." *Jahb. d. k. k. Geol. Reichs.*, xxxvii. (1887), 143. Translated in *Rec. Geol. Surv. India*, xxi. (1888), p 89. A similar boulder-bed is reported in South America, but on extremely imperfect evidence.

[2] "Salt Range Fossils." *Pal. Ind.*, ser. 13.

[3] *Rec. Geol. Surv. India*, xxv. (1892), p. 182.

it appears that the Ceratite beds probably include representatives of the Bunter, Muschelkalk and Keuper. Cephalopods are very abundant in the Bunter (where in Europe they are very rare); but are much less common in the upper beds. It is remarkable that, whereas ammonoids with true ammonitic sutures occur in the Productus Limestone, all the ammonoids from these Ceratite beds show ceratitic, or rarely goniatitic, sutures. In the Himalayan Trias, on the other hand, ammonoids with ammonitic sutures are common in the Trias.

In the Central Himalayas [1] the unconformity which is found in the Salt Range between the Lower and Upper Palæozoics is absent; and both Devonian and Carboniferous rocks have been described. But their fossils are yet unexamined.

The Carboniferous is overlaid—it is believed unconformably—by beds which are regarded as Permian and Trias; and of these the Trias has yielded an important series of fossils, which is now undergoing examination. The whole of the Triassic sequence appears to be present,[2] and the lower part, as in the Salt Range, is characterised by a great abundance of cephalopods. The higher beds show gradually increasing European affinities, until in the Rhætic portion of the series the deposits are said to be almost indistinguishable, both lithologically and palæontologically, from those of the Alps.

Finally before leaving the Upper Palæozoic and Lower Mesozoic rocks (which in India are naturally grouped together) it may be noted that Carboniferous fossils occur in Tenasserim, and have recently been described by Noetling.[3]

The Cretaceous rocks of India are not very extensive, but they are of great importance, for it is on them that

[1] Griesbach. "Geology of the Central Himalayas." *Mem. Geol. Surv. India*, xxiii. (1891).

[2] See a note by Mojsisorics, *Sitz. d. k. Akad. Wiss.*, Vienna, Math. Nat. Class. ci. (1892), translated in *Rec. Geol. Surv. India*, xxv. (1892), p. 186

[3] *Rec. Geol. Surv. India*, xxvi. (1893), p. 96.

Neumayr and others have based their view that Africa and India were at one time united by a continent which stretched across the Indian Ocean. The facts on which they rely are briefly these: The Cretaceous fauna of the north-west of the Indian Peninsula is very different from that of the south and east. The former is allied to the Cretaceous fauna of Europe; the latter closely resembles that of South Africa.

On the western side of India the chief area of Cretaceous rocks occurs in the valley of the Narbada near the turn of Bágh. Fossils are not very abundant, but they are of great interest. An attempt has been made[1] to show that the whole of the European Cretaceous from the albian to the senonian is represented; but the palæontological evidence is entirely insufficient to support this idea, and Oldham[2] returns the older view that the Bágh beds are all of cenomanian age. By far the greater number of forms which have been determined with certainty occur also in Europe.

On the east coast of India matters are entirely different. In the neighbourhood of Trichinopoly, Pondicherry and Viruddhachalam three distinct horizons are recognisable.[3] The lowest contains such typical cenomanian forms as *Ammonites rotomagensis;* the middle includes the turonian forms *Am. peramplus*, etc.; and the upper beds contain *Nautilus danicus*, *Inoceramus Cripsi*, *Crania ignabergensis*, which are characteristic of the senonian or danian. Thus the correspondence with the European sequence is fairly close; but in spite of this the greater number of the forms are absolutely unknown in Europe. Indeed out of a total of nearly 800 species of invertebrates found in South India, less than one-sixth occur in any European country. Simi-

[1] Bose. "Geology of the Lower Narbada Valley between Nimáwar and Káwant." *Mem. Geol. Surv. India*, xxi., pt. i. (1884).

[2] *Manual*, second edition, p. 251.

[3] See H. F. Blanford, "On the Cretaceous and other Rocks of the South Arcot and Trichinopoly districts, Madras," *Mem. Geol. Surv. India*, iv., pt. i. (1862); Stoliczka and Blanford, "The Cretaceous fauna of Southern India," *Pal. Ind.*

larly, on the most favourable view, out of forty species known in the Narbada area, only one-third are found in South India; and many of these are open to doubt.

On the other hand the Cretaceous of the Khasi hills between Assam and Sylhet is closely related to that of South India;[1] and nearly every species which is known in the former occurs in the latter. Moreover, both contain many South African forms. Out of thirty-five species of mollusca and echinodermata found in certain deposits in Natal, no less than twenty-two are specifically identical with forms of the South Indian Cretaceous.

It is thus clear that the Narbada Cretaceous is much more nearly allied to the Cretaceous of Europe than to that of Assam, South India or South Africa; while the faunas of these three places are closely related to one another. Hence it is concluded that South Africa, the east coast of India, and Assam lay on the south coast, and the Narbada area on the north coast of a tract of land which stretched across the Indian Ocean.

The Cretaceous rocks of Northern India are also interesting in another way. Although they are nearly allied to those of Europe yet they differ from the ordinary European development in this,—that there appears to be a complete passage from the Cretaceous to the Tertiary. In Baluchistan Oldham[2] has described a group of limestones with nummulites; and this group passes laterally into shales which contain Crioceras, Baculites, and Ammonites as well as nummulites. Doubt however has been cast on the details of this observation by Griesbach,[3] and the matter requires further observation. However this may be, the fact of a passage from Cretaceous to Tertiary is well authenticated in Sind.[4] The *Cardita beaumonti* beds contain a fauna

[1] Stoliczka in *Mem. Geol. Surv. India*, vii. (1869), p. 181, etc.

[2] "Report on the Geology of Thal Chotiali and part of the Mari country." *Rec. Geol. Surv. India*, xxv. (1892), p. 18.

[3] "On the Geology of the country between the Chapper Rift and Harnai in Baluchistan." *Ibid.*, xxvi. (1893), p. 113.

[4] W. T. Blanford. "The Geology of Western Sind." *Mem. Geol. Surv. India*, xvii. (1879).

which is neither definitely Mesozoic nor definitely Kainozoic, but is intermediate between the two.

We now come to the last, and perhaps the most important of all the questions which will be referred to here, *viz.*, the origin and age of the Himalayan chain. The subject is fully discussed by Oldham in perhaps the most interesting of all the chapters in the *Manual;* and this, at least, is a chapter which no geologist should omit to read. It is to Medlicott [1] that we owe our first conception of the way in which the Himalayan chain grew ; but it was the researches of Middlemiss [2] in Kumaun and Gahrwál which put the matter on a satisfactory basis and laid the real foundations of our knowledge of the process.

From a geological point of view the Himalayas may be divided into three zones which correspond more or less closely with the great orographical features of the range. The first is the Tibetan, which lies to the north of the chain of snowy peaks ; and in this belt marine fossiliferous rocks are well developed, but except towards the north-west end of the range they do not extend to the south. The second is the zone of the snowy peaks and of the Lower Himalayas to the south of them ; and these are made up of crystalline and metamorphic rocks, and of unfossiliferous sedimentaries (believed to be Palæozoic). And the third is the zone of the Sub-Himalayas or foot-hills, which form the margin of the range towards the Gangetic plain and which consist entirely of Tertiary, and principally of Upper Tertiary, rocks. It is the last zone which has been most carefully examined and concerning which we have the most accurate information. The rocks which compose it are divided into :—

Upper Tertiary or Siwálik,
Lower Tertiary or Sirmur.

The Lower Tertiary commences with a group of clays with some limestones and sandstones, in which marine

[1] *Quart. Journ. Geol. Soc.*, xxiv. (1868), p. 34 ; and *Manual*, first edition, chaps. xxii., xxiii.

[2] "Physical Geology of the Sub-Himalaya of Gahrwál and Kumaun." *Mem. Geol. Surv. India*, xxiv., pt. ii. (1890).

fossils occur; and this passes up with perfect conformity into the sandstones and clays of the upper part of the Sirmur series. The average coarseness of the deposit gradually increases in the upper beds; and this gradual increase of coarseness is maintained throughout the Upper Tertiary or Siwálik group until we reach the very coarse conglomerates of the Upper Siwálik.

West of the Jehlam the whole series is said to form a perfectly conformable sequence; and many of the sections in Kumaun and Gahrwál show a similar conformable passage from the Lower Tertiary to the Middle Siwáliks and from the Middle to the Upper Siwáliks. But this is not invariably the case; and in many places, especially towards the interior of the chain, the Upper Siwáliks rest upon the upturned and eroded edges of the Lower Siwáliks. Only one explanation is possible, namely, that the series of rocks was deposited during a period of disturbance, and that while continuous deposition went on in one area, in another the beds were raised and denuded and buried again.

Along the whole length of the Himalayan chain, wherever the Siwáliks are in contact with the pre-Tertiary rocks, the line of junction is a great reversed fault. In places this fault lies between the Lower Tertiaries and the older rocks; and in one area it lies between the Lower and Upper Tertiaries. Similar great boundary faults are not uncommon—indeed they are probably almost universal along the margins of great mountain chains;—but the peculiarity[1] of the Himalayan fault is that it is also a boundary of deposition. The Tertiary series never extended much to the north of the fault; for it is impossible to believe that the whole enormous thickness of these beds could have been removed so regularly and so completely as to leave no outliers.

Hence the conclusion is irresistible that during the deposition of the Sub-Himalayan Tertiaries the boundary of the Himalayan Range coincided approximately with this

[1] I am far from believing that this feature is really peculiar to the Himalayan Range; but it is more conspicuous and has been more clearly proved there than perhaps in any other chain.

great fault. The Siwáliks were laid down to the south of the fault, while the hills to the north were still rising ; and probably in part while the fault was still forming. Subsequently, the Siwáliks themselves became involved and were raised to form the present Sub-Himalayan Chain ; and now they bear the same relation to the alluvial deposits of the Gangetic plain that the older rocks to the north of the fault bore to them at the time of their deposition. Probably, too, there is now a boundary fault between the Sub-Himalayas and the Gangetic plain itself.

In Kumaun and Gahrwál, where the structure is well known, there is not merely one of these faults, but a whole series of parallel faults which divide the hills into a number of belts ; and in each belt the newest beds are older than the newest beds in the belt to the south and newer than the newest in the belt to the north. In each case the fault practically marks the northern boundary of the newest rocks in the belt to the south of it, and the age of these newest beds probably gives us the period at which the fault was completed.

Thus the Himalayas gradually grew outwards from north to south, slowly encroaching on the area of deposition to the south ; and each of these great reversed faults marks a stage in the process. Oldham goes on to show how this mode of growth agrees with Fischer's theory of the formation of mountains ; but for this discussion I must refer to the account given in the *Manual.*

The question of the age of the Himalayas also is too large a one to be dealt with here. It depends mainly on what is meant by the term Himalayan Chain ; and it must suffice here to point out that the presence of Nummulitic rocks at a height of 18,500 feet above the sea[1] clearly proves that at least a very large part of the elevation has taken place since the commencement of the Tertiary era.

PHILIP LAKE.

[1] La Touche. " Re-discovery of Nummulites in Zanskar." *Rec. Geol. Surv. India*, xxi. (1888), p. 160.

CONTINUOUS-CURRENT DYNAMOS.

PART I.

ALTHOUGH it is the alternate-current dynamo which of late years has shown the most rapid growth both in theory and use, yet its closely allied partner, the continuous-current machine, has not been without steady progress, especially in the field of practice; it may therefore be of interest to shortly review its recent history, and thence endeavour to forecast the direction towards which future advances will tend.

The earliest dynamo which was invented belonged to the so-called "uni-polar" or "non-polar" class in which there is no reversal of the direction of the E.M.F. generated in the active wires or "inductors" of the armature. In spite, however, of its invention so far back as 1831, the uni-polar type finds to this day little or no practical use. Visions of a perfect dynamo, in which a truly continuous current is produced and collected without any reversal of its direction, and therefore without any likelihood of sparking at the brushes, have frequently been revived, and the idea has always presented great attractions. Yet often as the cry has arisen for such a dynamo without a commutator and free from all the troubles incidental thereto, it has never assumed any very practicable shape, the difficulty of generating any considerable voltage within reasonable limits of size having proved an insuperable obstacle to the ingenuity of inventors.[1] The electrical engineer is thus thrown back on the bi- or multi-polar dynamo with its two principal methods of winding the armature, which are identified with the names of Gramme and Siemens. The theory of the dynamo was in the main worked out from the scientific study and practical use of these machines between the years 1870 and 1885; but with the date 1886 we reach a most important stage in its history. In the *Philosophical Transactions of the Royal Society* for that year was published the classic paper of

[1] For a recent examination of several different types of uni-polar machine, see *Elektrotechnische Zeitschrift*, 11th Aug., 1893.

Drs. J. and E. Hopkinson on "Dynamo Electric Machinery,"[1] — a paper which may be said to have raised dynamo-designing from rule-of-thumb empiricism to the level of a scientific art. In it the authors for the first time laid down clearly and concisely the true method by which the voltage of a machine might be predetermined from its constructive details, and so accurate was their reasoning that subsequent theorising has largely consisted in the explanation and expansion of their conclusions. It is therefore instructive to consider modern progress by the light of this remarkable paper, and we may conveniently use its several headings to furnish us with starting-points for our outline of more recent results.

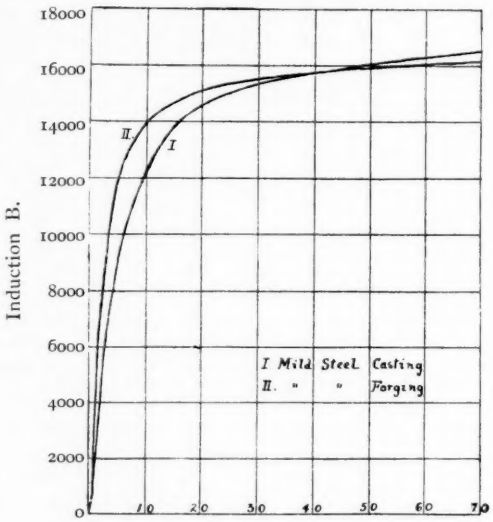
Its chief feature was an exhaustive analysis of the magnetic circuit of the ordinary bi-polar dynamo, and this led by the way to a consideration of the important question of the effect of the current in the armature on the magnetic field. Previous to it a number of formulæ had been invented for determining quantitatively the magnetism of a given iron circuit; one and all, however, had been tried and found wanting. The Drs. Hopkinson then took the designer back to the fountain source, Nature itself: determine by experiment the magnetic quality of the iron which is to be employed, and with the data thus obtained it will be possible to build up from the working drawings alone a complete predetermination of the voltage of the dynamo under any given degree of excitation. If the iron is the same as that experimented on, the answer must be correct.

The first essential then is a knowledge of our iron, and this now takes the form of "curves connecting B and H" for the particular class of iron used, or, in other words, curves from which for any given value of the magnetic difference of potential between opposite faces of a centimetre cube the number of C.G.S. lines which traverse it from one face to the other can be read off. Much against his will the iron manufacturer has been driven by the exigencies of competition to recognise

[1] Since reprinted in a collection of papers entitled *Original Papers on Dynamo Machinery and Allied Subjects* (Whittaker & Co.).

the necessity of having such curves constructed for his iron: fig. 1 shows two recent curves, the one for mild steel magnet forgings and the other for steel castings; the vertical ordinates represent magnetic induction and the horizontal abscissæ the corresponding magnetising force required to produce it. We are thus led to note the fact that improved metallurgical processes have brought about an apparent revolution in the nature of the material used for the magnets of dynamos. A few years ago, annealed wrought iron was the favourite material, and this was made of hand-picked scrap

FIG. 1.—Curves of exciting power and induction for mild steel.



Magnetising force H in C.G.S. units.

iron forged by hammering into large slabs. While hard steel was used for permanent magnets owing to its power of retaining its magnetism, the presence of steel in the forgings was regarded as well-nigh fatal to their magnetic value owing to its low permeability, and it was claimed that any admixture of this substance was best avoided by the process of hand-picking. Now, mild steel is freely used, and the question may be asked: How is it possible to reconcile present and past practice? The answer turns upon the vagueness of the term "steel"; in former days it

chiefly implied hard steel with a high percentage of carbon, a material which reaches its highest excellence in crucible cast steel. Gradually, however, the percentage of carbon has been so far reduced, and the softness so much increased that the same name of steel covers what is in effect as nearly pure iron as it is practically possible to produce. It is therefore mild or soft steel which is now so largely in use for dynamo work, and, so far as the purposes of the electrician are concerned, this is practically the same substance as was previously used in the form of wrought-iron forgings, the only difference being that whereas in the latter the iron was worked up into slabs by welding under the hammer, it is now produced in large quantities by melting in the furnace or converter and is then poured out into ingot or other moulds in which it solidifies. In the future we may expect such steel to play an ever-increasing part, especially for castings of a somewhat complicated shape which have also to serve as the carriers of magnetic lines.[1]

Given then the necessary information with regard to our iron, there follows an examination of the dimensions of the magnetic circuit of the dynamo which we are designing. The length and area of each portion is dealt with piecemeal, and the necessary exciting power required by it separately calculated; the final summing up of the various items so obtained enables us to draw a curve from which we can confidently predict how many useful lines will pass through the armature for any given value of the exciting power on the field-magnet.

The second important point of the paper of the Drs. Hopkinson was their treatment of the magnetic leakage in the case of two typical machines, the one of the Edison-Hopkinson type, and the other a Manchester dynamo with double horse-shoe field. In the first place, they experimentally determined the proportion which the lines straying beyond the limits of the armature bore to the number which actually traversed it, and were usefully cut by the wires on the surface: a factor (denoted by the symbol ν) was thus

[1] See a paper by Kapp, "On Methods of Testing the Magnetic Qualities of Iron," *Journal Inst. El. Eng.*, vol. xxiii., No. 110, 1894.

obtained, by which the number of useful lines has to be multiplied in order to determine the total number of lines passing through the field-magnet, and thence is calculated the density of the lines in the field-magnet. It was, however, pointed out that the value of the factor would vary according as the armature was more or less saturated, and shortly afterwards in the same year the magnetic leakage was dealt with in an even more complete manner by Mr. Kapp in a paper read before the Society of Telegraph Engineers [1] and also by Prof. Forbes in the discussion which followed. By the aid of their reasoning we are enabled to obtain a closely approximate figure for the number of leakage lines even when dealing with a new and untried type of dynamo. Numerous experimental determinations of the magnetic leakage in various makes of dynamos have since been published; [2] and much ingenuity has been expended in designing "iron-clad" dynamos, in which the field-magnet coils are enclosed in or shielded by iron in order to reduce the leakage to a minimum. It certainly is inconvenient to have our watches magnetised and our measuring instruments affected by dynamos at work near at hand; but experience has shown that it is easy to pay too high a figure in initial cost for the advantages of a dynamo with a small external field, except, of course, under special circumstances. One such circumstance is the use of large dynamos on board ship, where they are apt to disturb the compasses. This effect has manifested itself in a disagreeable manner in the case of the ships of H.M. Navy. Not only have the compasses been affected by dynamos, although situated at some distance away, but it is further stated that orders have been issued forbidding the carrying of bayonets by marines when on duty near working dynamos, since these iron arms become magnetised by induction, and then subsequently when brought near the compasses may deflect them. Quite recently the authorities of the British Admiralty have in-

[1] *Journal*, vol. xv., No. 64, 1887.

[2] *E.g.*, "Magnetic Leakage in Dynamos and Motors" (Ives), reprinted in the *Electrical Review*, 22nd and 29th Jan., 1892, and "Magnetic Data of Sprague Motor" (Parshall), *Electrical Engineer*, 13th June, 1890; also (Puffer), *Electrical Review*, 15th April, 1892

sisted on the dynamos constructed for their use being rendered practically innocuous in this respect by their being designed specially so as to have a negligible external field. In the case of small dynamos, which require proportionately small excitation, the difficulty may be met by the use of several comparatively thin iron screens enclosing the dynamo and separated from one another by intervening air-spaces. Such a device has been employed in a dynamo specially constructed for the Greenwich Observatory,[1] where delicate tests showed that its working did not affect the neighbouring instruments. When, however, we have to deal with large dynamos of the ordinary two-pole type, the remedy is by no means easily found, and it would seem that the only

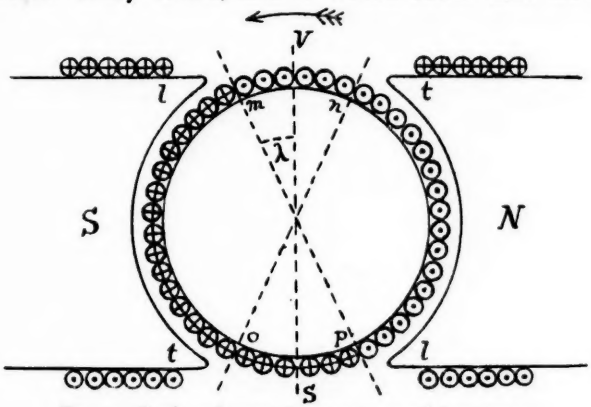


FIG. 2.—Back and cross Ampere-turns of Armature.

solution lies in a large reduction of the exciting power through the use of a very small air-space between the iron of the armature and the iron of the pole-pieces.

The next point on which again it is instructive to refer to the paper of Drs. J. and E. Hopkinson, is their examination into the reaction of the current in the armature upon the magnetic field of the dynamo. Considering for simplicity a bi-polar drum armature of the ordinary type, they showed that the actively inductive portion of the winding which lies on the surface of the core parallel to the pole-pieces has two magnetic effects which are of very different character. If λ (fig. 2) be the angle of lead of the brushes by which

[1] *Electrician*, 25th Aug., 1893.

they are displaced ahead of, or behind, the line of symmetry, VS, between the poles, the convolutions or loops included within the arcs *mn* and *op* have a direct magnetising effect, which will either increase or decrease the total number of lines passing through the armature according as the current-sheets embraced between the points *m*, *n* and *o*, *p* flow in the one or the other direction. The remaining convolutions included within the arcs *mo*, *np* were shown to produce "a material disturbance in the distribution of the induction over the bored face of the pole-piece": if the edge of a pole by which an inductor first enters into its field be called the "leading" pole-tip as opposed to the "trailing" tip by which it leaves the pole, then the lines are driven away from the leading corners *ll* and are crowded up towards the trailing corners *tt*. As, however, the weakening of the field at the leading pole-tip is counterbalanced by the strengthening of the field at the trailing pole-tips, "this disturbance has no material effect upon the performance of the machine". In the ordinary dynamo the brushes are advanced ahead of the line of symmetry in order to avoid sparking, *i.e.*, assuming the direction of rotation shown in fig. 2, to a position corresponding with the line *mp*. In this case, if the poles be as shown in the diagram, the current in the wires *m* to *n* is directed towards the observer as indicated by the dotted circles, and away from the observer in the corresponding wires to *op* as shown by the crossed circles. On comparing their directions with the direction of the current in the neighbouring wires of the magnet winding proper, it will be seen that they are exactly opposed to each other: in other words, the magnetising effect of the loops between *mn* and *op* is opposed to the magnetising force of the fixed coils on the magnet. They are, in fact, "back ampere-turns" neutralising an equivalent number of the forward ampere-turns of the stationary magnet-coils. On the other hand, the loops between *mo* and *np* are "cross ampere-turns" distorting the field by twisting it round in the direction of rotation. This division of the armature ampere-turns into two sets was brought into

greater prominence by Mr. Swinburne:[1] he showed that whatever the exact nature of the winding, the wires lying on the surface of the core may be regarded as united across the ends of the armature by imaginary connections into two sets of loops, lying in planes at right angles to each other. The subject was again advanced in two papers,[2] the one by the last-mentioned author and the other by Mr. Esson, until now the terms "back" and "cross" ampere-turns of the armature have become part of the orthodox terminology of the dynamo designer.

The final result of the various theoretical points to which brief allusion has been made above, may be summed up in the statement that the electrical engineer is now enabled to design a dynamo to produce a given voltage at a given speed with such accuracy that, when built and tested, its performance will not vary by more than two per cent. from that predetermined by calculation: such accuracy is obtained in common practice and within the limits of time that the busy routine of a large engineering factory permits of being given to the design.

The truth of the above-mentioned analysis of the effect of the armature current on the magnetic field was fully substantiated in another paper by Dr. Hopkinson appearing in the *Proceedings of the Royal Society*, 15th Feb., 1892. In this sequel to his former paper he gave an account of some experiments, in which the induction at different parts of the air-gap under the bored face of the pole-piece was measured. Taking two points, the one near the leading corner and the other near the trailing corner, he showed that within the limits of experimental error, the difference in the induction at those two points corresponded exactly to the difference in the magnetising forces due to the cross effect of the armature current which was flowing at the time of the experiment.[3] It was thus proved that, provided the iron

[1] *Journal Soc. Tel. Eng.*, vol. xv., No. 64, p. 542, Nov., 1886; also "Constant-speed Motors," *Electrical Engineer*, 20th Jan., 1888.

[2] *Journal Inst. El. Eng.*, vol. xix., part lxxxv.

[3] See also a paper "On the Relation of the Air-gap and the Shape of the Poles to the Performance of Dynamo-electric Machinery" (Ryan), *American Inst. El. Eng.*, 2nd Sept., 1892.

pole-pieces be far from saturation, the total number of lines passing through the armature is not affected by the cross magnetising force of its current. If, however, the pole-pieces have insufficient area and are therefore near saturation, the distortion of the lines may result in an actual diminution of their number owing to the trailing pole-corners being unable to carry the increased number which is crowded into them. This danger can, however, be obviated by proper design.

The real importance of the cross reaction lies in the fact that it weakens the fringe of lines which extends from the leading pole-tips. It is in this fringe that the section of the winding short-circuited by the brushes must be reversed, and the stronger the current which it is carrying, the stronger must be the field to reverse it; yet by the very action of the armature current the reversing field is weakened. It has thus been established that if the process of commutation is to be carried out successfully without sparking, the cross ampere-turns of the armature at full load[1] must not bear too great a proportion to the ampere-turns of the field-winding which are expended over the air-gap. Roughly it may be said that experience shows the advisability of not allowing the cross ampere-turns to exceed half the ampere-turns required by the air-gap, or, to put it in another way, the induction at the leading pole-tip must not be reduced to less than about 2500 or 3000 C.G.S. lines per sq. cm., *i.e.*, one-half of the average density over the whole of the polar air-space which usually ranges from about 5000 to 6000. Various methods have been suggested and tried for neutralising the effect of the cross ampere-turns of the armature,[2] but the only principle that can be said to be used in practice consists in keeping the length of the air-gap so large that the excitation which it requires is more than double the cross ampere-turns of the armature.

[1] *Journal Inst. El. Eng.*, vols. xix., part lxxxv., p. 95, and xx., part xciii., p. 268.

[2] See "On the Reaction of the Armature Current on the Field" (J. Fischer-Hinnen), *Elektrotechnische Zeitschrift*, 3rd Feb., 1893, and *Electrical World* (Ryan), 19th Nov., 1892.

This brings us to the great question of multi-polar *versus* bi-polar dynamos for large outputs. Given an armature core of a certain size, its output, so far as regards voltage and the current that may be obtained from it without overheating, is, roughly speaking, independent of the number of poles. As, however, we increase the number of poles, the cross ampere-turns acting on each pole are reduced in proportion: if therefore the number of poles be raised from two to four the length of air-gap required to prevent sparking may be reduced to half of that which would be required in the bi-polar machine.[1] Herein lies the advantage of the multi-polar type, but the question when the output is too large to be successfully obtained from a two-pole dynamo cannot be said to have been finally decided as yet. This point will be again alluded to later, and the fact is merely noted here that multi-polar forms have found much more favour on the Continent than in Britain.

The concluding remarks of the present article will be devoted to the efficiency of modern continuous-current machines. Viewed as a converter of energy from one form into another, the dynamo must certainly be pronounced the most wonderful machine which the engineer has at his command. Eight years ago, when Dr. Hopkinson determined by experiment the efficiency of an Edison-Hopkinson machine, he was able to show that when mechanical energy was imparted to the shaft of the armature at the rate of 57·6 H.P., electrical energy was delivered at the terminals in a useful form at the rate of 53·7 H.P., or an efficiency of over 93 per cent. In the waste power is included the loss due to the passage of the current through the resistance of the armature, the energy required for the excitation of the magnet, and the mechanical loss by friction of the bearings, and when we consider that all these losses must necessarily be present, however much we may succeed in minimising their amount, it will be seen how little room there is for improvement on the score of efficiency. Hence, in point of fact,

[1] "Notes on the Design of Multi-polar Machines" (Esson), *Journal Inst. El. Eng.*, vol. xx., part xciii.

although the efficiency above recorded has often been repeated and is now guaranteed under ordinary commercial conditions, it has seldom been surpassed, at least in machines of similar comparatively small size. The method devised by Dr. Hopkinson for testing the efficiency of dynamos is worthy of passing mention. Two similar dynamos are taken and are connected together either by a coupling or by belting; one of these, A, is arranged to work as a generator, and the current which it gives is supplied to the other, B, which consequently works as a motor. The mechanical energy delivered by the latter supplies the greater part of the driving power required by A; the horse-power of B as a motor must, however, necessarily be less than the horse-power required to drive A as a generator. This deficiency has, therefore, to be made up from some external source of power, and in the original experiments of Dr. Hopkinson it was supplied through a transmission dynamometer, which at the same time measured it. Thus the whole system is self-supporting, save for the necessary losses in the two machines under test, and from the power supplied to make up these losses, the efficiency of the two machines is easily deducible. The beauty of the method lies not only in the fact that, *e.g.*, two dynamos of fifty horse-power each may be tested under full load by means of a ten horse-power engine, but also in the directness with which the waste power is measured. A subsequent improvement of the same method consists in the use of a small subsidiary dynamo to supply the waste power electrically to the circuit: all the measurements can then be made electrically by voltmeter and ammeter, and a very high degree of accuracy is thus obtained.[1] When the dynamos of 240 kilowatts output at the Manchester central electric light station were tested by this method, efficiencies of 93·7 and 95·2 per cent. were found to have been obtained.[2] Of late years, owing to the demand for dynamos coupled directly to the crank-shafts of their driving engines for central-station work, much

[1] "The Determination of the Efficiency of Dynamos" (Kapp), *Electrical Engineer*, 22nd and 29th Jan., 1892.

[2] *Electrician*, 6th Oct., 1893.

attention has been paid to the over-all efficiency of the combined steam-engine and dynamo, or the ratio which the useful output at the terminals of the dynamo bears to the horse-power given out by the steam in the engine cylinders as shown by indicator diagrams. Two well-authenticated instances may be cited to show the present pitch of excellence. In a high-speed engine indicating 300 horse-power and directly coupled to a dynamo having an output of 1500 amperes at 120 volts, tests made at the Naval Exhibition of 1891 in London showed an over-all efficiency of 84 per cent., and on the assumption that the brake H.P. of the engine was 90 per cent. of the indicated H.P., an efficiency is found for the dynamo of 93·5. With a similar engine of 200 I.H.P. coupled to a dynamo for 125 volts and 1025 amperes at 350 revolutions per minute, an over-all efficiency of 85·6 has been obtained, the dynamo alone having the remarkable efficiency of 95·6 per cent. On the Continent, where large multi-polar machines running at considerably lower speeds are largely used, the over-all efficiency of the combination is generally about 82 per cent., this somewhat lower figure being accounted for partly by the slower speed and partly by the fact that the numerous poles demand a greater expenditure of exciting energy than would be the case if their lines were concentrated into a single magnetic circuit.

Finally, it is worth remarking that such high efficiencies as have been mentioned above are not obtained at the expense of the all-important quality of durability. It is often said that an extra 1 per cent. in the efficiency is dearly bought at the sacrifice of strength in some part of the machine's frame. But this reasoning, true though it is in many ways, loses much of its force when applied to the case of dynamos, and for a reason which is easily explicable. All the electrical and magnetic losses manifest themselves in the end as heat, and so tend to raise the working temperature of the dynamo: now one great guarantee that a machine will be durable is that its temperature remains low, so that its insulation is not likely to deteriorate from the effects of heat even after many years of hard work. Hence,

when the designer succeeds in reducing one of these losses, he is also rendering it easier to produce a dynamo which keeps cool in continuous work, and which is therefore more likely to be durable. A striking instance of this may be seen in the precautions which are now taken to eliminate eddy or parasitic currents, whether in the armature core itself or in the winding. In large machines the armature bars are now very commonly composed of several separate strands, lightly insulated from each other, twisted together and gently compressed into a rectangular section.[1] By this construction any single strand continually passes from one side to the other of the bar: hence, if the field in which the bar is moving varies in density within the limit of its breadth, each strand is partly in the stronger field and partly in the weaker. Thus no one strand produces a higher E.M.F. than any other, and the tendency for a local current to be set up within the bar when it enters or leaves the field of a pole is entirely eliminated.

C. C. HAWKINS.

[1] Crompton's patent, No. 12,880, 1886. For a valuable method of experimentally measuring the amount of the eddy currents in a wound armature, see *Electrician*, vol. xxvi., 1891, pp. 699, 700 and 740, and vol. xxvii., p. 162.

(To be continued.)

ON THE MORPHOLOGICAL VALUE OF THE ATTRACTION-SPHERE.

PART I.

PROBABLY no more rapid biological advance has ever been made than that which is at present gradually bringing into view the extraordinary structural complexity of a single cell. The ceaseless energy with which workers of all nations have attacked the cell problem will leave a standing monument to the value of anticipatory theories (however visionary) as a stimulus to research. Indeed it is through such speculative promptings that, as Whitman (1) lately put it, "all the search lights of the biological sciences have been turned upon the cell. It has been hunted up and down through every grade of organisation, it has been searched inside and out, experimented upon and studied in its manifold relations as a unit of form and function. It has been taken as the key to phylogeny and ontogeny, and on it theories of heredity have been built." Nor has the consequent increase of knowledge been confined to any one department of cytological research; but has affected the whole range of cellular existence, beginning in the protozoa and ending with the complex tissues and formed material into which cell-structure breaks down in the higher differentiation of the compound types.

Among other things it has been made abundantly evident that the rough though time-honoured division of a cell merely into nuclear and cytoplasmic constituents is no longer sufficient, and must be revised in accordance with the study of cellular development, while it is at the same time strangely interesting to see that under the influence of this new study the truth of old conceptions has not been lost, but reappears in the ever-increasing importance we attach to the view, that all cell-structures, however complex, can in their origin be resolved into nuclear and cytoplasmic derivatives, for such a conception is now the almost universal

key-note of cellular morphology. Much labour has been spent in attempting to determine a boundary line between such nuclear and cytoplasmic series proceeding on the assumption that structures outside the nucleus are cytoplasmic, whether they persistently exist or arise in that position, so long as they are not produced from dismembered portions of the nucleus, the inverse of this definition being applied to structures which are nuclear.

In its simplest form the cytoplasm (protoplasm) presents under a sufficiently high magnifying power the familiar, fine reticulum which has been interpreted in many ways. Perhaps the simplest, and certainly the most comprehensible, hypothesis respecting its undifferentiated structure is contained in Bütschli's (2) conception of a "Schaumplasm," *i.e.*, it consists of an immense number of fluid globules immersed in a non-miscible fluid, so that in optical or actual section it presents the appearance of a fluid reticulum. Under the action of stains, the meshes of this net-work colour less deeply than the strands, the stain being absorbed by fine granules (microsomes) along their course, while the fluid of which they are composed remains comparatively clear. The manner of coloration of some of the microsomes is similar to that of the interior nuclear mass, which also in its normal condition is built on the same general plan, *i.e.*, when stained, it consists in optical section of a reticulum, the colour being absorbed here by the chromatin granules, which are immersed in clear threads of fluid (linin). All cells show this gross structural division into nuclear and cytoplasmic groups, and although either may be more or less masked by the evolution of secondary structures during ontogeny, they form the ground-work of cellular organisation through every phase of life-history and metamorphosis. They are the elements out of which these superadded structures are evolved, and into which they again break down at the close of individual existence. Of late years, however, much less conspicuous, and less easily demonstrable cellular constituents, have been held by many, as probably of the same prime order. It is almost exactly twenty years since the able and vanished naturalist Hermann Fol (5) drew attention to

appearances of radiation round a point in the eggs of certain jelly-fish (geryonidæ) as being worthy of remark: "Setzen wir aber gerade in diesem Augenblicke etwas Essigsäure hinzu, so kommt der Rest, gleichsam, nur eine Andeutung des früheren Kernes wieder zum Vorschein. Auf beiden Seiten dieser Kernüberbleibsel zeigen sich zwei Protoplasmaanhäufungen, deren dicht angesammelten Körnchen zwei regelmässige sternförmige Figuren darstellen. Die Strahlen dieser Sterne werden durch die in geraden Linien aneinander gerichten Körchen gebildet. Mehrere solchen Linien reichen von einem Stern oder Anziehungscentrum in einem Bogem zum andern, inden sie die Reste des Keimbläschens umfassen. Das ganze Bild ist äusserst klar und deutlich und errinnert lebhaft an die Art und Weise wie ausgestreuter Eisenstaub sich um die beiden Pole eines Magneten anordnet. An den Ränden gehen die Strahlen allmählig einerseits in das dünnere Protoplasmantze des Endoplasmas, andererseits in das dichte Ectoplasm über."

For a long time the existence of these appearances attracted little more than desultory comment, and it was not until fourteen years had passed in the gradual improvement of our methods of research, that the classical publication of van Beneden (7) revolutionised our cytological conceptions by showing that similar radial appearances were of prime importance in the early ontogenetic development of Ascaris and were probably to be regarded as cellular constituents of the highest rank. Not content, however, with merely demonstrating the existence and functional importance of his "sphères-attractives," the Belgian naturalist conferred upon them the title of "Organs of the cell," each in itself composed of several parts. For if eggs be killed and stained by the admirable, if somewhat heroic, method he adopted, the spheres present a conspicuous three-zoned appearance, caused by the presence of a central body, surrounded by a light space, and enclosed on the outside by a broad band of granular protoplasm. Under the adopted treatment the central body stains bright green, a little darker than the rest of the sphere, but it is not in the least distinct in staining capacity from numer-

ous other granules existing elsewhere in the egg. It has, in fact, as far as its optical properties go, all the characters of these "microsomes," being distinguished from them during divisional metamorphosis solely by the position which it occupies at the centre of affairs.

The sphere in Ascaris was the first to be minutely studied and described, and it has consequently served as a sort of type for comparison with those we are becoming familiar with on all sides. It is essentially composed of a large mass of differentiated protoplasm outside the nucleus, containing within itself a clear space and central body.

The spheres are known in the literature which deals with them under a great variety of names, "sphère-attractive" being practically synonymous (among others) with "Sphère-directrice," "Aster," "Archoplasm," "Polar-body," "Polar-corpuscle," and their German equivalents, "Peryplastes," with their "Daughter-peryplastes," "Microcentrums," "Spermocentres," "Ovicentres," "Nebenkerns," "Astrocentres," "Vitelline nuclei" and "Dotterkerns". But although these titles have arisen in the description of obviously homologous structures found in a great variety of eggs and tissues, they by no means always refer to the whole, or even to the same part of the three-zoned structure originally described by van Beneden. Thus the centrosome is always the central staining granule, and is exactly equivalent to "Polar-body," "Polar-corpuscle," "Daughter-peryplaste," and "Microcentrum" (Heidenhain's (8) term for multiple centrosomes in the aggregate). But how far "Ovicentres," "Spermocentres," "Astrospheres" and "Astrocentres," "Vitelline nuclei" and "Dotterkerns" represent this and nothing more, it is difficult to say, while such terms as "Sphère-directrice," "Aster," "Archoplasm," "Peryplaste" and "Nebenkern" certainly stand for the whole sphere, which, as in Ascaris, generally contains within itself a smaller clear space and centrosome. The terminology of Boveri (9) has the advantage of simplicity, since by his use of "Archoplasm" for the outer, and "Centrosome" for the inner constituent of the sphere, no mistake is likely to be made. But as the archoplasm and the centrosome

can exist quite separate from one another, even during rest, it would be well to find some term to comprehend all these structurally co-related parts wherever they may be situated in the cell.

In the following pages I have employed the term "Attraction-sphere" in this sense, shortly, "Sphere," reserving "Archoplasm" for the outer granular mass to which it was originally applied and "Centrosome" for the inner granule.

Whether, with van Beneden, we are to regard the spheres as organs of the cell depends a good deal upon what is meant by the term organ. In its original conception the word has a physiological significance, and in this sense the purposive nature of any structure constitutes its claim to be regarded as an organ. Consequently both the nucleus, cytoplasm, and all other structures functional in the maintenance of the cell, fall into the category of organs, the spheres with their very definite functions among them. That they are not so regarded by many histologists is nevertheless a fact; but this tardiness in the acceptance of van Beneden's views has really nothing to do with indefinite terminology arising from certain considerations respecting the *origin* of the spheres of quite a different nature.

It has become a biological postulate, engrained by the invariable nature of the evidence on which it rests, that a cell in its origin is possessed of a nucleus and cytoplasm, if nothing else. And it is equally certain that Virchow's aphorism, "*omnis cellula e cellula*," could be extended down to the origin of the cytoplasm from pre-existing cytoplasm, and the nucleus from pre-existing nuclei of previous generations. But this simple duality of structure, with which cells often start in life, may, and very generally does, give place to a greater complexity of parts, through the development of accessory structures, of which some Nebenkerns form typical examples. Whatever be the function of these bodies (and in many cases it is not known), they are often plainly derived, by direct metamorphosis, from either the nuclear or cytoplasmic substance. They do not come in with the prime constituents at the outset, but are

evolved during the course of cellular ontogeny. On the other hand the nucleus and cytoplasm, as such, are never so evolved. Their relative proportions may increase or diminish without limit, but they neither arise *de novo*, nor are they ever entirely suppressed, themselves constituting the raw material out of which any future structural complexity is evolved. Until lately, the difference of structure apparent in the presence of the nucleus and cytoplasm was the only cellular organisation of which anything definite was known, and as both these constituents are typically *persistent*, it became customary to regard cell-organs as things always arising directly by fission from pre-existing similar structures. In other words, we have acquired the habit of regarding *persistence* as characteristic of cellular organs. No sooner, therefore, was the presence of spheres demonstrated than the question presented itself—Do these structures arise by division from pre-existing spheres, or can they, unlike the prime factors, be evolved during cellular ontogeny? And though this question of their origin is of the utmost importance, it appears to me that with respect to the nature of the spheres as cell-organs it has in reality nothing whatever to do.

It remains then to determine if possible whether the spheres (quite apart from their character as organs of the cell) have thus an *evolved* or *persistent* nature. How do they arise in the sexual elements? Are they persistent, but restricted to the male, dying out in the ova; persistent, but restricted to the female, dying out in the spermatozoa? Or do they arise from both, and fuse in the egg like the nuclei in the formation of a completely fertilised cell?

Finally, can these organs ever be formed anew, and so lose their claim to persistence and a place among the prime factors of the cell?

Van Beneden regarded the spheres as arising simultaneously, immediately after the entrance of the spermatozoa into the egg: "Les deux sphères apparient simultanement, si parfois on croit n'en voir qu'une, cela depend de la position relative des deux organs relativement à l'observation". In this case they do not appear to be present at the outset

either in the ova or in the spermatozoa, but to arise immediately after fertilisation, as the first organs evolved during individual ontogeny. The increasing amount of evidence which bears directly on this point has produced no other result so far than that of equally increasing the already wide divergence of opinion respecting the origin of the spheres in the first instance. Some observers, like Vejdovsky (10) (in the case of Rhinchelmes), believe that they enter with spermatozoa already pre-formed into the egg, having apparently arisen from pre-existing spheres in the spermatogenesis. Julin (11) regards the possession of a sphere as a distinctive character of the male cell of Stylopsis grassolaria, and goes so far as to make this a mark of distinction between ova and spermatozoa in general.

The researches of Balbiani (12) show the Dotterkern, "Vitelline nucleus" of Arachnid eggs, to be homologous with what he terms the "Nebenkern (Centrosome of Platner)," *i.e.*, of the attraction-sphere of these eggs, and to arise from the *nucleus!* as a little bud, which, although sometimes dividing, rapidly becomes "hypertrophid" and functionless, constituting a homological stepping-stone between those forms where the sphere appears to be present solely in the male, and those in which it is present only in the female.

Boveri in his last publication maintains that there are neither centrosomes nor asters related to the mitoses which form the polar bodies in the ovigenesis of Ascaris, and he uses this observation to support his theory that the spheres are derived entirely from the male.[1]

In the spermatogenesis of certain mammalia and elasmobranchs I (13, 14) have found bodies answering to the centrosomes to be incorporated with the mature spermatozoa, and Field (16) seems to have arrived at similar conclusions with respect to the spermatogenesis of echinoderms. Fick (17) figures a sphere related to the intruding spermatozoon

[1] While at the Naples laboratory last spring, my friend Dr. Morton Wheeler showed me preparations of the eggs of myzostoma with beautifully defined centrosomes at the apices of the polar-body spindles, so that Boveri's view is not of universal application.

in the fertilisation of the eggs of Axolotl, while indirectly Hermann (18) and others have arrived at similar conclusions by showing the "Mittelstück" to be a derivative of the Nebenkern in several spermatogenetic series, a structure which there is every reason to believe to be the archoplasmic portion of the attraction-sphere. There can indeed be very little doubt that sometimes at any rate spheres do pass into the ovum from the male as the descendants of pre-existing spheres in the antecedent generations of the spermatogenetic series. Others, however, like van Beneden, still hold that the spheres can be evolved in the egg, without any intervention on the part of the spermatozoon! and in the case of Ascaris they have not yet been shown to be derived from pre-existing spheres in any way.

And there is still a third, or intermediate class of observers, who conclude from the types they have examined that the spheres are produced in the first instance from both male and female cells alike, being presumably derived from pre-existing spheres in the spermato- and ovogenetic series, "tous les astrocentres du descendant, étant dérivés par divisions successives des astrocentres primitifs, se trouvent provenir, par parties égales, du père et de la mère," says Fol (6), who, in his famous and much-criticised "Marche du quadrille," figures an aster and centrosome related to each pro-nucleus in the fertilised eggs of sea-urchins, maintaining that each sphere divides before the formation of the first segmentation spindle, one-half of his "Spermocentre" uniting with one-half of his "Ovicentre" to form a new attraction-sphere at each end of the first spindle-figure.

More recent observations, like those of Konklin (20) on the zoological, and Guignard (21) in his notable paper relating to the fertilisation of Lilium Martagon, on the botanical side, have arrived at substantially the same results; although it is possible that the figures supposed to be indicative of fusion in these cases admit of other interpretations.

A vast importance attaches to the supposed fusion of the spheres, since it not only presupposes persistence in both sexes for these structures but suggests that they may have

as important functions in relation to hereditary phenomena as the nuclei themselves. In order, however, to really claim this for the spheres it would be necessary to find at least as strong evidence for their universal bi-sexual origin and fusion during fertilisation as we have in the case of the nuclear elements. Yet it is sufficiently apparent that nothing of the kind is forthcoming, that it is impossible to formulate any rule having the faintest pretence to be general respecting the uni-sexual or bi-sexual origin of the spheres; and we are equally baffled by the want of unanimity in recorded observations when we try to determine whether the spheres in their origin are always derivatives of pre-existing spheres. Of the two alternative answers to the first question, however, one must be true. Either the spheres have an altogether mixed origin, and can arise indifferently from the male or female, or be evolved in the egg at the time of fertilisation, and are *ipso facto* excluded from any hereditary value whatever, or those observations which tend to show that they originate in this way are incomplete, and with more extended knowledge their bi-sexual nature and fusion will be established throughout. Nor should it be forgotten that the evidence which militates against this bi-sexual origin is of one kind only. It consists of the non-forthcoming of one or other of the figures of that "Marche du quadrille," as Fol originally described it; and it appears consequently at first sight to be negative.

With respect to the persistent or evolved nature of the spheres, it will, moreover, be obvious that these structures may be persistent, although arising from one parent only; and the only direct evidence against their persistence in this sense is that which supports the view that they are evolved *de novo* in the egg, and is at present very slender. Undoubtedly attraction-spheres are brought in by the spermatozoa in some species of animals, whatever may be the case in plants, while bodies answering to them are incorporated with the male element during spermatogenesis or its equivalent in others. Evidence which is really positive has greater weight than any amount of that which is merely negative, and on the strength of this, probability seems to set towards

the persistent as well as bi-sexual nature of the spheres. But on closer inspection of the facts it does not seem to me that the evidence against persistence is really altogether of a negative character. If the spheres have an altogether mixed origin arising from either sex or being evolved *de novo* in the egg, the appearances would be exactly those which have been hitherto recorded. Consequently these can be and have been used as positive evidence of a mixed origin. Nor can there be any doubt that the attractive speculations which sprang from the suggestive observation made by Fol, and its startling confirmation in Guignard's paper which followed close upon its heels, have exercised an inevitable tendency in swaying the minds of observers towards a confirmation of these views which are legitimate only if the "Marche du quadrille" is the tune to which the centrosomes invariably "dance" through the initial phases of ontogeny. There are, however, other sources of evidence bearing on these vexed questions, of which I shall treat successively; one involving our knowledge of the life-history of the spheres in successive generations of tissue-cells, and another the as yet too scanty observations relating to the same phenomena in several protozoan animals and plants.

J. E. S. Moore.

(To be continued.)